

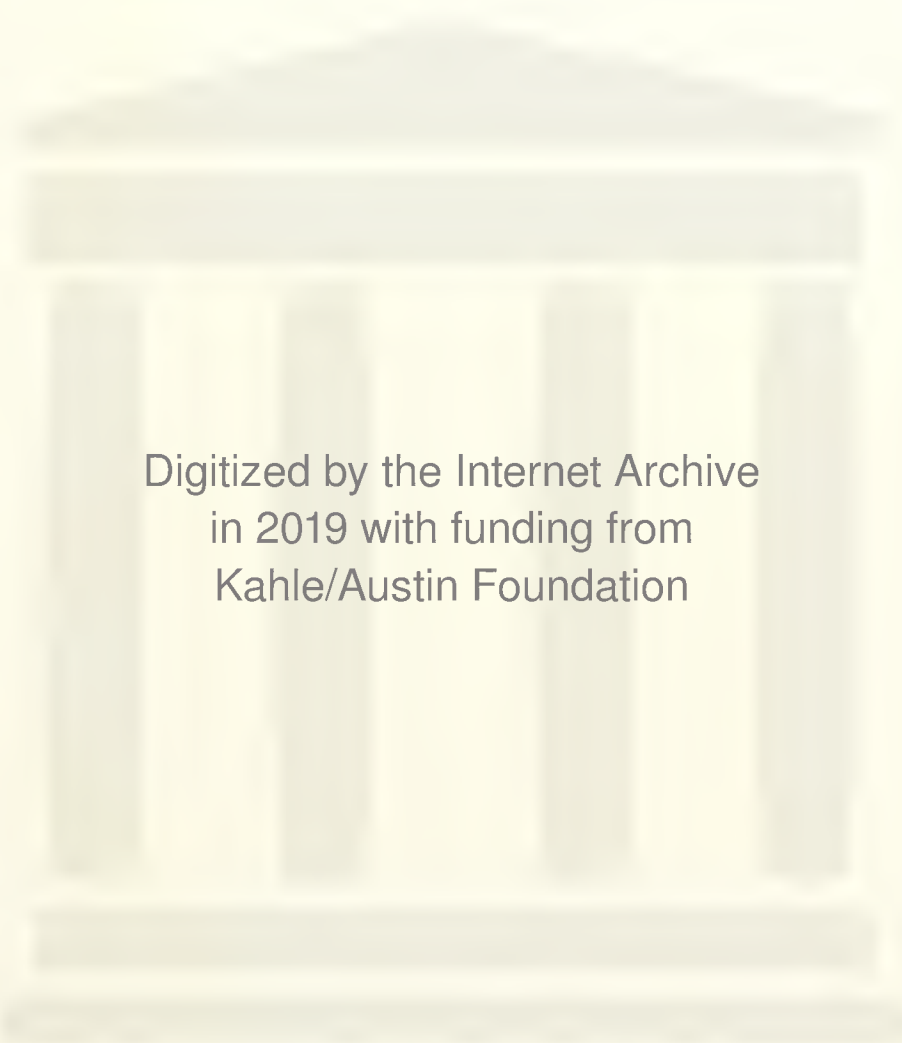
Biographical Memoirs

NATIONAL ACADEMY OF SCIENCES

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Biographical Memoirs

NATIONAL ACADEMY OF SCIENCES

NATIONAL ACADEMY OF SCIENCES,
OF THE UNITED STATES OF AMERICA

Biographical Memoirs

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Biographical Memoirs

VOLUME XL





E. T. Allen

EUGENE THOMAS ALLEN

April 2, 1864–July 17, 1964

BY CHARLES A. ANDERSON

EUGENE THOMAS ALLEN was one of the pioneers in the field of geochemistry in the early part of this century, and much of his illustrious career was with the Geophysical Laboratory of the Carnegie Institution of Washington, from which he retired in 1933. He celebrated his centennial on April 2, 1964, and died on July 17, 1964, in Arlington, Massachusetts, where he had lived for many years following retirement. His last days were brightened by the greetings and flowers sent to him on the occasion of his 100th birthday. His death came rather suddenly and was quiet and peaceful, following a week of illness from which he appeared to be recovering.

Dr. Allen was born in Athol, Massachusetts, the son of Frederick and Harriet Augusta (Thomas) Allen, both descendants of old New England families. His father was engaged in a number of successive mercantile and manufacturing enterprises in Massachusetts. E. T. Allen was married in Arlington, Massachusetts, to Harriet Doughty of Arlington on August 26, 1896. Mrs. Allen had been born in Charlestown, Massachusetts, on January 8, 1865, and died in Arlington on February 11, 1945.

Dr. Allen received his A.B. degree from Amherst College in 1887, followed by graduate study in chemistry at The Johns

Hopkins University, where he received his Ph.D. degree in 1892. While working for his doctorate, he was Associate in Chemistry at the Women's College of Baltimore, now Goucher College. He spent the academic year 1892-1893 as Acting Professor of Chemistry at the University of Colorado, then studied at Harvard University for two years. For six years (1895-1901) he served as Professor of Chemistry at the Missouri School of Mines.

Allen's first contribution to geochemistry was made at Missouri. It involved a study of terrestrial native iron found in three localities in sedimentary rocks associated with coalbeds. Careful analytical work, coupled with concern for the geologic environment—qualities that persisted throughout Allen's career as a geochemist—proved that the iron was not meteoric or artificial but truly one of the rare natural occurrences of this element in sedimentary rocks.

Because of Dr. Allen's long career with the Geophysical Laboratory, it seems appropriate to give some of the history leading to the establishment of this outstanding organization. Shortly after the creation of the U.S. Geological Survey in 1879, Dr. G. F. Becker, together with Dr. Carl Barus, began to make systematic physical and chemical studies of igneous rocks. Apparatus was developed for the measurement of high temperatures and other properties. Nearly ten years were consumed in the preparation of this equipment, but in 1892 appropriations were curtailed and the laboratory work was discontinued. Dr. Becker persisted in his efforts for restoration of funds, and in October 1900 he was able to bring Dr. Arthur L. Day from the Physikalische Technische Reichsanstalt to reactivate the program. In 1901 Dr. Allen joined the staff of the Geological Survey to begin his long association with Dr. Day.

Day and Allen started work on the feldspars—the most abundant of the rock-forming minerals. The first step was the development of equipment for accurate high-temperature ther-

mometry and the measurement of the melting points and thermal properties of the plagioclase feldspars. The melting points of these feldspars were determined with an accuracy never obtained before, and these points, considered with reference to their composition and the series of specific-gravity determinations on chemically pure feldspars made by Allen and Day, settled beyond question that albite, the sodium feldspar, and anorthite, the calcium feldspar, form a continuous series of isomorphous crystals. A comparison of the data of Allen and Day with Roozeboom's classes of isomorphous mixtures shows that the plagioclase feldspars fit Type I of Roozeboom's classification. By analogy, Allen and Day were able to explain the zonal structure of plagioclase found in thin sections of most igneous rocks.

In order to keep the Geological Survey's Geophysical Laboratory working at a high level of efficiency, the Trustees of the Carnegie Institution of Washington supplemented the Congressional appropriations by awarding grants to Dr. Becker and to Dr. Day; the later stages of the feldspar program were completely financed by the Institution. In appreciation of the splendid financial support given this program, the Director of the Geological Survey authorized publication of the feldspar study by the Carnegie Institution of Washington in 1905 (Publication 31).

By this time it was apparent that Congressional appropriations were not sufficiently stable to encourage an expanded and vitally needed program in geophysics and geochemistry, and the Carnegie Institution of Washington established the Geophysical Laboratory in December 1905, with Arthur L. Day as Director. The wisdom of this step has been amply demonstrated by the brilliant record of the staff of physicists, chemists, and geologists working together in a concentrated attack on fundamental geological problems.

Dr. Allen transferred to the Geophysical Laboratory in 1906

and became a key member of several teams working in collaborative research. His career with the Laboratory can be divided into three parts. For several years he continued working on the preparation of artificial silicates and on their melting temperatures and stability fields. In the middle part of his career, he worked on various metal sulfides that are of importance in ore deposits. The last part of his career was devoted to the fundamental problems of volcanic activity, particularly as related to the source of the water and gases involved in volcanic eruptions, geysers, fumaroles, solfataras, and hot springs.

In the work on silicate minerals, Allen collaborated with F. E. Wright, J. K. Clement, and W. P. White—Dr. Wright performing most of the petrographic studies that aided in the determinations of the various crystals formed during crystallization. The first study in this program was on MgSiO_3 , but only one crystalline form was obtained in definite crystals, clinoenstatite, with a melting point of 1521° . This magnesian pyroxene is found in certain meteorites and rarely in igneous rocks. In addition, three other different types of fibrous, crystalline aggregates were obtained, but modern X-ray diffraction techniques have proved necessary to determine their identity—techniques not available to Allen and his associates at the time.

Allen next studied the calcium silicates, wollastonite and pseudo-wollastonite. He was successful in synthesizing wollastonite by preparing a glass of the composition CaSiO_3 and heating the glass to 800° - 1000° . Both natural and artificial wollastonite, on heating to 1180° , changed without melting to the hexagonal form, pseudo-wollastonite. The reverse change could not be brought about except by the addition of other substances to reduce viscosity. Pseudo-wollastonite is rarely found in rocks but is a common constituent of some furnace slags. This study is important in petrology because it gives limiting temperatures for the formation of wollastonite, a common metamorphic rock mineral.

Allen next turned to tremolite, chemically the simplest known amphibole, consisting of calcium and magnesium metasilicate. The attempts failed to crystallize tremolite by heating glass of tremolite composition below 1000-1100°, or by rapid cooling of the melt of the proper composition. This failure suggested to Allen that tremolite must be hydrous and that water is essential for its crystallization. He then carefully analyzed five natural tremolites and found that the H₂O content ranged from 1.72 to 2.5 percent. Dehydration studies on tremolite revealed only small losses in water until a temperature of 850° was reached, and Allen suggested that the H₂O, an essential constituent in the tremolite, was dissolved in solid solution. By modern techniques we now know that (OH) is an integral part of the tremolite structure; Allen's work was the first indication that tremolite requires more than Ca, Mg, and SiO₂ for its formation.

Allen's last study on the synthesis of silicate minerals was on diopside, one of the simpler pyroxenes composed of calcium and magnesium silicate; this more complex system followed naturally after his studies on the CaSiO₃ and MgSiO₃ compounds. His experiments with these two end members demonstrated that each end member has dimorphous forms, each with a separate melting point. The inversion of CaSiO₃ at 1190° is to pseudo-wollastonite (in this paper Allen refers to it as β wollastonite in contrast to α wollastonite, the lower temperature form and the common mineral). The β form of MgSiO₃ is magnesium pyroxene (clinoenstatite), and below 1365° it is transformed to the lower temperature orthorhombic form which is distinct from enstatite, the naturally occurring mineral. The only stable form that appeared in the experiments was diopside (CaSiO₃.MgSiO₃), with a melting temperature of 1380°. A eutectic occurs between diopside and β wollastonite at the composition 60 percent diopside and 40 percent CaSiO₃, with a melting temperature of 1348°. This was one of the first

experimental studies to demonstrate the complexity that may exist in a simple system of two end members.

About 1910, A. L. Day and R. B. Sosman of the Geophysical Laboratory were involved in perfecting improved high-temperature thermometry, and Allen contributed to this study by carefully analyzing a number of metals—Pb, Co, Ni, Cu, Ag, Al, Sb, Zn, and Cd—in order to determine their purity for use as fixed points. The high quality of these analyses is regarded as one of his finest contributions as a chemist.

Allen was attracted to the problem of ore deposition because of the scarcity of chemical data to help the geologist understand the complex processes of ore formation. In the early stages of this program, Allen was aided by John Johnston, J. L. Crenshaw, H. E. Merwin, and E. S. Larsen, Jr. The sulfides of iron were their first targets and Allen was able to demonstrate from his experimental work that the orthorhombic FeS_2 , marcasite, forms only at low temperatures in association with free acid, and cannot exist above 450° . The isometric FeS_2 , pyrite, forms in alkaline or nearly neutral solutions and is stable at low as well as high temperatures. The mineral pyrrhotite, Fe_{1-x}S , is of variable composition, the amount of sulfur in the mineral depending on the pressure of the sulfur vapor. Allen pointed out that the nonmagnetic FeS , troilite, is the end member of the pyrrhotite series and is not a distinct mineral species—a conclusion that is accepted today by mineralogists.

In Allen's next study on some of the metal sulfides, he emphasized that the geologic portion of the subject of mineral genesis should be left to the geologist, but that decided advantages can be derived from active consultation between the experimentalist and the field worker, a point of view that is just as valid today as when Allen made it in 1912. This second study involved the two forms of ZnS , isometric sphalerite and hexagonal wurtzite, and the comparable forms of HgS , hexagonal

cinnabar and isometric metacinnabar. Experimental work on these sulfides brought out the same relations that were determined for pyrite and marcasite, namely, marcasite, wurtzite, and metacinnabar form only from acid solutions, whereas pyrite, sphalerite, and cinnabar are the common primary minerals in nature and crystallize from alkaline or nearly neutral solutions. This concept greatly advanced our understanding of the formation and stability of these important metal sulfides.

In 1913 L. C. Graton of Harvard University organized the so-called Secondary Enrichment Investigation under the joint auspices of copper companies, the Geophysical Laboratory, and Harvard University. The main emphasis was on the copper minerals. Allen, with his colleagues at the Geophysical Laboratory, Eugen Posnjak, H. E. Merwin, E. G. Zies, and R. H. Lombard, contributed the fundamental knowledge of the physical chemistry involved in the secondary enrichment.

The first paper concerned with this program was a systematic study of copper sulfides and dealt with two simple minerals, chalcocite (Cu_2S) and covellite (CuS). The dimorphic character of chalcocite was proven, the orthorhombic form being stable at low temperatures and the isometric form at higher temperatures. The inversion point of 91° was accepted, although coarsely ground chalcocite had an inversion point of 105° , which subsequent work has demonstrated to be the correct temperature. Solid solution of CuS in chalcocite was demonstrated up to 8 percent of covellite, causing darker colors and lower specific gravities of the chalcocite.

The composition of bornite (Cu_5FeS_4) was in doubt, and Allen selected five samples of unquestioned purity for chemical analysis, confirming the composition given above. The data of four of these samples are given in the Seventh Edition of Dana's *System of Mineralogy*.

Probably the most significant paper from the Geophysical

Laboratory resulting from the Secondary Enrichment Investigation was published in 1916 based on work by Allen and his two associates, E. G. Zies and H. E. Merwin. The reactions of a number of natural sulfides with copper sulfate solutions were investigated quantitatively, special attention being given to chalcocite (Cu_2S), covellite (CuS), bornite (Cu_5FeS_4), chalcopyrite (CuFeS_2), pyrrhotite (Fe_{1-x}S), pyrite (FeS_2), sphalerite (ZnS), and galena (PbS). In all of these experiments, a copper enrichment product was formed—either a sulfide or, in special cases, metallic copper and cuprite. It was demonstrated that cuprous sulfate acts more quickly than cupric sulfate in replacing the original sulfide by chalcocite, the most important copper mineral found in supergene enriched deposits. It was emphasized that the quantities of ferrous iron and cuprous copper that can exist in solution are very small as compared with the quantities of ferric iron and cupric copper, but if the ferric iron could be removed *pari passu* with the precipitation of the cuprous copper, the reduction of cupric copper by ferrous iron could proceed unhindered. Geologically, this is extremely important, because it implies that if limonite (hydrated ferric oxide) is present with supergene chalcocite, the chemical reaction took place above the water table; but if limonite is absent, the formation of supergene chalcocite took place below the water table—an implication that is substantiated by abundant field evidence.

In 1917 Allen and R. H. Lombard devised a new method for the determination of dissociation pressures at comparatively high temperatures for use in the study of sulfide minerals. The dissociation pressure curves of covellite (CuS) and pyrite (FeS_2) were determined from 1 mm to 500 mm pressure. This method supplies a convenient means for the synthesis of sulfides in cases where the dissociation of the sulfides causes difficulty. The study was the last that Allen made under the auspices of the Secondary Enrichment Investigation.

During World War I the United States was forced to develop its own supply of optical glass, and Allen with his associate, E. G. Zies, contributed greatly to the methods of analysis and made studies on the condition and role of arsenic in optical glass. Here, as in all of his research, Allen demonstrated the skill that made him one of the great analysts of his time.

Lassen Peak erupted in June of 1914, the first outbreak in this century of an active volcano within the boundaries of the United States (Alaska was still a territory). Dr. Allen and A. L. Day visited the volcano in 1915 to start a program of physical and chemical measurements of its activity, to be followed in 1922 and 1923 by extensive studies of the hot springs in the area. This marked the beginning of Dr. Allen's third major interest with the Geophysical Laboratory, volcanic phenomena, which continued throughout the remainder of his career. Allen's excellent review of volcanic-gas analyses became a standard reference. In addition, he made some preliminary tests of gases at Sulphur Banks at Kilauea Volcano and joined with E. G. Zies in a classic chemical study of the fumaroles in the Valley of Ten Thousand Smokes near Katmai. This last study showed that by 1919 the fumarolic activity was declining and that steam was the predominant constituent of all gases, the remaining gases consisting of HCl , CO_2 , H_2S , N_2 , HF , associated with minor amounts of O_2 , CO , A , and NH_3 . It was concluded that the water was largely of surface origin that was heated and vaporized by the still-hot rocks before it entered the fumaroles.

The studies of Lassen Peak and its hot springs were published in the admirable memoir by Allen and Day. The eruptions at Lassen Peak culminated in May 1915 when the crater was filled with viscous dacite; then mud flows followed, and a horizontal hot blast devastated the northeastern flank of Lassen Peak. Careful studies of the hot springs proved that the alteration of the surrounding lavas was caused by sulfuric acid. Two

types of alteration were observed, one producing kaolin and some opal, and the other producing opal with alunite (K-aluminum sulfate). Kaolin provides the key as to the type of alteration, because it is decomposed by strong sulfuric acid. The sulfuric acid forms near the surface by oxidation of the H_2S contained in the rising gases. If sufficient water percolates through the rocks, the concentration of the acid will be kept low, permitting the kaolin to form. However, if the sulfuric acid forms in nearly dry ground, the acid may reach such high concentration that kaolin will be unstable.

Steam wells and associated hot springs at "The Geysers" in the northern Coast Ranges of California were studied by Allen and Day in 1924 and 1925. This was the pioneer scientific study of the hydrothermal areas that have since become important for geothermal energy. The excellent temperature-pressure data gathered by Allen and Day, plus the fact that several of the wells were allowed to "blow" for thirty years, were major reasons for renewed interest in "The Geysers" and culminated in 1960 in the construction of the first geothermal power plant in North America. At the present time, geothermal energy as a source of power is utilized in Italy, New Zealand, Iceland, the United States, Japan, the USSR, and the Congo, and is becoming of increasing world-wide interest.

In 1925 Allen and Day started a five-year study of the hot springs and geysers of Yellowstone National Park—a fitting climax to their joint attack on the hot spring problems. The result is a beautiful and monumental monograph, copiously illustrated with pertinent photographs, filled with a wealth of data, and spiced with their interpretations. This monograph is a vital reference for all students of geysers, hot springs, and fumaroles.

The highlights of the research on the hot spring problems are succinctly stated by Allen in his papers "Neglected Factors in the Development of Thermal Springs" and "Geyser Basins

and Igneous Emanations.” Geological factors such as topography, water supply, and character of the local rocks are of fundamental importance. Hot springs depositing travertine (CaCO_3) exclusively are probably always associated with limestones. Allen was one of the first geochemists to recognize that alkali chlorides form a considerable proportion of the mineral matter in hot waters of the alkaline type. He suggested that the halides were transported from a magma, but the actual means of transport were in doubt because the physical chemistry of this problem was not understood. A pioneering effort was made in Yellowstone to take physical measurements of the discharge of hot water from different areas; these were the first quantitative heat flow measurements from a large hot spring area.

The career of E. T. Allen illustrates the point that great advances can be made in complex subjects like geochemistry and geophysics by working in a favorable environment and joining in collaborative research with other specialists. There must also be insistence on carefully controlled experimentation and lucid presentation of results. Allen’s writings bring out these qualities and also indicate the inherent honesty of the man.

Allen was a very modest man, and the last to boast about his “Mayflower” background. He was kindly, with a smiling face, and always helpful to the younger men around him. In the early days of the Geophysical Laboratory, the Petrology Club met regularly in Whitman Cross’s home, near the Laboratory. Whitman Cross, J. P. Iddings, Henry S. Washington, Arthur L. Day, and E. T. Allen were regular members, plus younger men from the Geophysical Laboratory and the U.S. Geological Survey. One might say the “giants of petrology” were the leaders of the group, and discussions were frequently acrimonious. In these episodes, E. T. Allen would break in to make a point in

his quiet, friendly manner, always bringing a more peaceful atmosphere to the meeting.

Allen never lost his interest in geochemistry, and when his old colleague and friend, George W. Morey, sent him a Christmas note in 1960 saying that he was planning to study the hot springs at Yellowstone the following summer, Dr. Morey received a three-page handwritten letter outlining in some detail the work that Allen thought should be done, concluding with the following statement: "I would love to go out there with you, but I am afraid that my doctor would take a dim view of this. Perhaps he is right; I will be 97 in April."

Dr. Allen was elected to the National Academy of Sciences in 1930. He was a member of the American Chemical Society and President of the Washington Section in 1904. He was a Fellow of The Geological Society of America and of the American Geophysical Union.

ACKNOWLEDGMENTS

Dr. E. G. Zies of the Geophysical Laboratory was most helpful in furnishing material for this memorial and in reviewing the manuscript. My colleagues, Michael Fleischer and Donald E. White, have been equally helpful. Harold E. Stevens, Counsellor-at-Law in Boston, Massachusetts, kindly sent me background material on E. T. Allen's family.

THE WRITER of this biographical memoir has been a great admirer of E. T. Allen because much of Allen's work has dealt with topics in his own professional line of interest. It has been a matter of deep regret that he never had the opportunity to know Dr. Allen personally. Because of his high regard for Dr. Allen and warm appreciation for his help and guidance at a critical stage in his career, the writer is delighted to have had the opportunity to prepare this biographical memoir.

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COMPILED BY MICHAEL FLEISCHER AND E. G. ZIES

KEY TO ABBREVIATIONS

Am. Chem. J. = American Chemical Journal

Am. J. Sci. = American Journal of Science

Am. Mineral. = The American Mineralogist

Carnegie Inst. Wash. Publ. = Carnegie Institution of Washington
Publication

Chem. News = Chemical News and Journal of Industrial Science

Econ. Geol. = Economic Geology

J. Am. Ceram. Soc. = Journal of the American Ceramic Society

J. Am. Chem. Soc. = Journal of the American Chemical Society

J. Geol. = Journal of Geology

Z. anorg. Chem. = Zeitschrift für anorganische und allgemeine Chemie

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Walter H. Bender

WALTER HERMANN BUCHER

March 12, 1888–February 17, 1965

BY W. H. BRADLEY

WALTER BUCHER was a man of almost unbounded enthusiasm, curiosity, and infectious good humor. The most common reaction on meeting him was a feeling of stimulation, whether it was a first meeting or the fiftieth. He had an eager and frank curiosity about nearly every aspect of natural history—an eagerness that was contagious. But this interest centered on dynamics: how and why the crust of the earth moved; how plants and animals lived and what they accomplished in the scheme of nature. Indeed, Bucher's interests reflected the buoyant flow of his own restless energy.

Bucher's father and his paternal grandfather and grandmother came from Switzerland, in or near Zurich. His mother and her parents all came from Württemberg in southern Germany, though all three lived at one time or another in Zurich. Without exception, all six of Bucher's immediate forebears were strongly religious, his mother almost fanatically so. Bucher's father and mother were both very fond of music and so were several of the grandparents. He inherited this love of music and was a skilled pianist. Rather curiously, the strong family inclination to religion seems to have touched him lightly. As a youngster he was much exposed to books and an intellectual atmosphere. His father was a scholarly man who had

a special interest in, and aptitude for, languages, especially Latin, Greek, and Hebrew.

Bucher's father came to the United States when he was only seventeen years old, and lived first in Wheeling, West Virginia, and later at Akron and Cincinnati, Ohio. Walter was born in Akron but when he was five years old he and his parents moved to Frankfurt-am-Main, where his father had been sent by the Methodist Church to teach Hebrew, Greek, and homiletics in a missionary school. As a consequence, Bucher acquired all his schooling in Germany, including four years at the University of Heidelberg, from which university he received his Ph.D., *summa cum laude*, in 1911. He entered the university to study zoology and paleontology, but under the influence of Professor Wilhelm Salomon-Calvi, he changed to paleontology and geology. After completing his university work he took a brief course in English, which by that time had become virtually a foreign language to him.

Bucher returned to the United States in 1911 and established residence in Cincinnati. He spent most of his time for the next two years at the University of Cincinnati attending lectures in geology and paleontology and improving his English. His gift for teaching and stimulating students must have been readily apparent, for he was soon asked to lecture on a volunteer basis. In 1913 he became a member of the faculty as an instructor. Thereafter he was appointed successively Assistant Professor (1915), Associate Professor (1920), Professor (1924), and, in 1937, Research Professor and Chairman of the Geology Department.

In 1914 Bucher married Hanny E. Schmid, the daughter of a close family friend. She survives him. There are four children, all married. One of them, John Eric Bucher, is a petroleum geologist. The other son, Robert W., and the two daughters, Mary Dorothy (Mrs. John Plunkett) and Margaret (Mrs. R. G.

Oellers), and their families live in New Jersey near the parental home.

Although never a student of Bucher's, I can readily understand what an exciting experience that must have been. I came to know him in the twenties through a mutual friend. We realized at once that we shared many interests in the fields of geology and natural history. For the next decade or so, I benefited greatly by reason of his interest in my research. With characteristic generosity he made his experience and knowledge freely available to me. He also made me acutely aware of the wealth of information available in the European literature, if I would only gain some facility with the languages.

Bucher was Chairman of the Geology Department at Cincinnati only three years. He left there to become Professor of Geology at Columbia University with the understanding that he was to specialize in structural geology. This, of course, was most agreeable to him because by this stage in his career he had already become deeply involved in the major tectonics of the earth, on which subject he had published many papers and his justly celebrated book, *The Deformation of the Earth's Crust*.

Bucher's published papers and books may be divided into two quite different categories. Those published in the interval between 1911 and 1919 have to do with paleontology and with the origin and significance of such minor features of sediments as stromatoliths, ripple marks, and oolites. These are important contributions to our understanding of the present-day processes that give rise to these features and that help explain similar structures found in sedimentary rocks almost from the beginning of the geologic record. They reveal his restless quest for understanding how things come to be.

Beginning in 1920 with the publication of his paper "The Mechanical Interpretation of Joints," the focus of his interest shifted to structural geology, where it remained, almost but not

quite exclusively, for the rest of his life. Moreover, his interest shifted rather soon into two aspects of structural geology, cryptovolcanic structures and the major deformation of the earth's crust.

Bucher was the first in this country to call attention to, and map systematically, some of these remarkable, nearly circular, complex structures known as cryptovolcanic structures. He was the first also to compare them with similar features in Europe. Curiously, his geologic mapping of two of these structures, Jeptha Knobs in Kentucky and Serpent Mound in Ohio, represents the only systematic geologic mapping he did, yet he had the keenest interest in seeing geologic features in the field for himself rather than depending solely on geologic maps made by others. Bucher explained these features as the result of gas explosions caused by intense heat rising from cupolas of igneous magma that never rose all the way to the earth's surface.

In later years many of these cryptovolcanic structures have been interpreted as the results of meteor impact, but Bucher remained highly skeptical of this interpretation. Indeed, his last published paper was a timely and carefully reasoned plea for caution against uncritically adopting the conclusion that all such features are of meteoritic origin.

Bucher's greatest contribution to geology came from his long-continued study of the major structural features of the earth's crust. His objective was to understand the physical properties of the earth's crust and the forces that operated to deform it. Much of his reasoning was based on analogy with the major structural features exposed in present-day mountain ranges, though he supplemented this with laboratory experiments which he designed and conducted himself.

In 1933 he published his conclusions in a book, *The Deformation of the Earth's Crust*, which was at that time a significant milestone in geologic thought. As he recognized himself,

parts of it were already out of date before it was published. I think it fair to say that one important cause of that was because his earlier papers on the same subject had stimulated others to concern themselves with one aspect or another of this major problem. Another, and probably greater, reason was that the geophysicists were making great strides in determining the physical properties of the earth's crust, especially at great depths below the surface.

In an Anniversary Day address before the Geological Society of America (1938) entitled "Deformation of the Earth's Crust," he brought his own thinking up to date and summarized the work of many others in a significant and still very valuable contribution to our knowledge of the major tectonics of the earth.

Had he lived just a few years more he surely would have been much excited by the enormously long, rectilinear features that are now being found on the ocean floor and, locally, on the continents. These large and apparently geologically very ancient structural features are strikingly reminiscent of the proportionately long, nearly rectilinear breaks he observed in his experiments with shrinking spheres.

As his bibliography shows, he continued to publish occasional papers dealing with paleontology, sedimentary structures, and geomorphology.

Perhaps two of the chief values of Bucher's works were the great stimulation they aroused in other earth scientists and the fact that he brought to Americans the conclusions and ideas of many Europeans, for he had an amazing grasp of the whole European literature.

Bucher was first of all a teacher of rare ability. But, in addition, he was a very productive research scientist. He engaged in both activities with his characteristic ebullient enthusiasm until he retired in 1956. During the latter part of his

career at Columbia he served as Newberry Professor of Geology. John T. Rouse and Charles H. Behre, Jr., in a memorial to Bucher published in the *Bulletin of the Geological Society of America* (in press), have written an appraisal of Bucher's role as a teacher at some length and far better than my information permits. Their excellent appraisal, fortunately, reveals as much about Bucher's warm and colorful personality as it does about his quality as a teacher.

After retirement Bucher became part-time consultant to the Humble Oil and Refining Company, and spent about half of each year at their laboratory in Houston, Texas. He died of heart failure while on duty there on February 17, 1965.

He was widely recognized as a leading figure in geology and was awarded a considerable number of honors and honorary positions. The responsibilities involved in the latter he discharged with his usual vigor. The following statement of these honors is quoted, with minor alterations, from Rouse and Behre's memorial cited above.

"Bucher was elected President of the Ohio Academy of Sciences in 1935 and to membership in the National Academy of Sciences in 1938. He became chairman of the Division of Geology and Geography of the National Research Council for the term 1940-1943 and was President of the New York Academy of Sciences in 1946. He was Vice President (1948) and President (1950-1953) of the American Geophysical Union. In 1953 he served as Vice President of Section E of the American Association for the Advancement of Science. In 1954 he was elected Vice President and in 1955 President of the Geological Society of America, which Society he had served earlier as Councillor (1935-1937).

"He was a Fellow of the American Academy of Arts and Sciences, an Honorary Member and Foreign Correspondent of Société Géologique de France, and an Honorary Member of

Société Géologique Belgique and of the Deutsche Geologische Gesellschaft.

“In 1955 he received the William Bowie Medal of the American Geophysical Union, in 1955 the Leopold von Buch Medal of the Deutsche Geologische Gesellschaft, and in 1960 the Penrose Medal of the Geological Society of America.

“Honorary doctor degrees were awarded him in 1947 by Princeton University, in 1957 by Columbia University, in 1962 by Durham University, England, and in 1963 by the University of Cincinnati.”

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KEY TO ABBREVIATIONS

- A.I.M.E. Tech. Publ. = American Institute of Mining and Metallurgical Engineers, Technical Publications
Am. Assoc. Petroleum Geol. Bull. = American Association of Petroleum Geologists Bulletin
Am. J. Sci. = American Journal of Sciences
Bull. Geol. Soc. Am. = Bulletin of the Geological Society of America
Econ. Geol. = Economic Geology
Geol. Rundschau = Geologische Rundschau
Geol. Soc. Am., Spec. Pap. = Geological Society of America, Special Paper
J. Geol. = Journal of Geology
J. Wash. Acad. Sci. = Journal of the Washington Academy of Sciences
Proc. Geol. Soc. Am. = Proceedings of the Geological Society of America
Trans. Am. Geophys. Union = Transactions of the American Geophysical Union
Trans. New York Acad. Sci. = Transactions of the New York Academy of Sciences

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Hugh L Dryden

HUGH LATIMER DRYDEN

July 2, 1898–December 2, 1965

BY JEROME C. HUNSAKER

AND

ROBERT C. SEAMANS, JR.

HUGH LATIMER DRYDEN was born in Pocomoke City, Maryland, July 2, 1898. His father taught school and later kept a general store. This business failed in the panic of 1907 and the family moved to Baltimore, where the father became a streetcar conductor, following this occupation for the rest of his life. Young Hugh attended public schools and a high school, called Baltimore City College, graduating in 1913 just short of age fifteen.

Entering The Johns Hopkins University with advanced standing, he completed a regular B.A. curriculum in three years, receiving his degree with honors in 1916 and his M.A. in 1918.

It is of interest to observe that Dryden did not come from a scholarly family. But he was endowed with the highest order of intelligence, brought this gift to the realms of physics, engineering, and government service, and developed a vigorous philosophy supported by strong Christian principles.

He married Mary Libbie Travers on January 29, 1920, and their three children were highly educated. The son, Dr. Hugh, Jr., an organic chemist, graduated from Hopkins and M.I.T. The elder daughter, Mrs. Mary Ruth Van Tuyl, graduated

from Goucher College and is married to a mathematician at the Naval Ordnance Laboratory. Daughter Nancy Travers graduated from American University and teaches school in Montgomery County, Maryland. There are five grandchildren.

PERIOD OF ACTIVE RESEARCH

In June 1918 Dryden joined the staff of the National Bureau of Standards as an inspector of munitions gauges, intending to return to graduate school on a fellowship in the fall. However, because of World War I and with the encouragement of Dr. Joseph S. Ames, head of the Johns Hopkins Physics Department and Chairman of the National Advisory Committee for Aeronautics, his plans were changed. He obtained a transfer into the Bureau's newly-formed wind tunnel section. After Dr. Ames arranged to give courses to a number of Hopkins graduate students at the Bureau, Dryden was able to complete his thesis work on experiments carried on after hours in the wind tunnel. He was granted the Ph.D. in physics in 1919, when he was just under twenty-one, the youngest student ever to obtain a Ph.D. at Johns Hopkins. Dryden's thesis, entitled "Air Forces on Circular Cylinders," addressed itself to the fundamental problem of scale effects on the flow over circular cylinders normal to the wind. Its value lies in an early demonstration of the fact that Reynolds' Number based simply on a characteristic dimension of the body is not always the sole criterion for aerodynamic flow similarity. His results stimulated some of the more sophisticated inquiries into the same subject in the decade which followed.

In 1920 Dryden was placed in charge of the wind tunnels. Here his research on the problems of wind tunnel turbulence and boundary-layer flow brought him international recognition. Dryden and his colleagues were first interested in accurately measuring turbulence in wind tunnels and in understanding its effects on force measurements. It became apparent

to them early that the hot-wire anemometer provided a means of direct measurement of turbulence. The instrument in its original form could not, however, follow rapid fluctuations. Dryden and his colleagues devised an electrical network which restored the loss in amplitude and compensated for the lag. Extensive tests were made of the intensity and scale of turbulence produced by the wire screens at various distances from the working station. Having means for varying the intensity and scale of the turbulence and for measuring these quantities with a compensated hot-wire anemometer, Dryden built wind tunnels of very low turbulence and measured on models the effect of turbulence on aerodynamic forces. The experimental work showed the effect of turbulence on the transition from laminar to turbulent flow in the boundary layer near a solid surface. In NACA Technical Report 342 by Dryden and Kuethe, a curve was presented by which measurements of the air resistance of spheres could be interpreted to give the turbulence quantitatively. It was shown that discrepancies which had previously been observed in tests on standard airship models were mainly due to differences in the turbulence of the tunnel in which the tests were made. The theoretical equations of laminar flow within a boundary layer had been previously announced by Prandtl in 1907. Dryden and his collaborators were able experimentally to verify Prandtl's theories. They emphasized the practical importance of maintaining a laminar boundary layer over as much of the surface of the aircraft as possible in order to reduce drag.

Dryden summarized at various times the work of the Bureau of Standards group on turbulence and boundary layer, as in his Wright Brothers Lecture of 1939, entitled "Turbulence and the Boundary Layer." His most recent summaries of this subject were in his paper presented at the National Congress of Applied Mechanics in June 1958, entitled "Some Aspects of Boundary-

Layer Flow in Subsonic and Supersonic Air Streams," and another paper published in the *Journal of Applied Mathematics and Physics* (Vol. IXb, 1958), in honor of the sixtieth birthday of Jacob Ackeret, entitled "Combined Effects of Turbulence and Roughness on Transition."*

In collaboration with Dr. Lyman J. Briggs, Dryden made some of the earliest experimental measurements of the aerodynamic characteristics of airfoils at high speeds. The early motivation for this work had its origin in the effects of the high propeller tip velocities which were being encountered with high-powered engines. Dryden and Briggs carried out these investigations at a large compressor plant at the Edgewood Arsenal. Through this work, they furnished the propeller designer with airfoil data at high speeds and developed early insight into the effects of compressibility on lift coefficient and pressure distribution. They were among the first to observe experimentally the so-called "transonic drag rise." This early work was supported and published by NACA. NACA Technical Reports 207 (1924), 255 (1926), 319 (1929), and 365 (1930) by Briggs and Dryden summarize these experiments. Interest generated by this work led to the construction of many high-speed wind tunnels and was of pioneering significance when jet and rocket propulsion made supersonic and hypersonic flight feasible.

Although Dryden's career at the Bureau of Standards is characterized largely by his research on turbulence and boundary-layer flow, his inquiring mind led him to grapple with other engineering problems with many different collaborators. His investigations of wind pressures on chimneys, mill buildings, and skyscrapers laid the basis for rational design of structures subjected to wind loads. Dryden's principal collaborator in this field was G. C. Hill. Examples of their work are contained in Bureau of Standards Research Paper 545 entitled "Wind Pres-

* A listing of Dryden's papers may be found in "Publications of Hugh L. Dryden," NASA Historical Note (March 1966).

sure on a Model of the Empire State Building" (1933) and Bureau of Standards Research Paper 221 entitled "Wind Pressure on Circular Cylinders and Chimneys" (1930).

Dryden guided a three-year program to pave the way for introducing modern materials and construction methods in low-cost housing. This little-known chapter in his career resulted in Bureau of Standards Building Materials and Structures Report BMS 1 entitled "Research on Building Materials and Structures for Use in Low-Cost Housing" (1938).

Dryden's studies of turbulence led him naturally to an interest in mechanical vibrations. His collaborator here was L. B. Tuckerman. They published Bureau of Standards Research Paper 556 in 1933 entitled "A Method of Exciting Resonant Vibrations in Mechanical Systems" (also with H. B. Brooks) and Bureau of Standards Research Paper 678 in 1934 entitled "A Propeller-Vibration Indicator." The motivation for this work was undoubtedly the strong concern during the early 1930s for the structural integrity of propeller blades under increasing speeds and disc loadings.

A summary of Dryden's scientific and engineering research would be incomplete without mention of his interest in the measurement of the acceleration of gravity. This work took place during 1942 and 1943 and resulted in Bureau of Standards Research Paper 1502 entitled "A Reexamination of the Potsdam Absolute Determination of Gravity" (1942) and "Absolute Gravity Determinations" (published in *Transactions of the American Geophysical Union*, Vol. 24, 1943). This investigation, done in collaboration with E. A. Eckhardt, W. D. Lambert, and A. H. Miller, undertook to study the various determinations of the absolute value of gravity and to recommend a "best value." The results indicated that only three determinations had been made with sufficient attention to the elimination of systematic error to merit consideration.

Dryden was responsible for extensive studies of the aerody-

namics of aircraft bombs and for the development of a practical method of designing the tail fins to ensure aerodynamic stability. With E. J. Lorin, a form of bomb geometry was standardized that remained in use for many years. His less-known contributions ranged over aircraft noise, ventilating fans, aerodynamic design of aircraft control surfaces, automobile streamlining, and aerodynamic cooling. He authored, for example, the division on "Aerodynamics of Cooling" in the well-known volumes on *Aerodynamic Theory*, Vol. VI, Division T (Springer, Berlin, Germany, 1936).

As time passed, Dryden's management responsibilities at the Bureau of Standards grew and he found less time for his own research. In 1934 he became Chief of the Mechanics and Sound Division. With the establishment of the National Defense Research Committee and later the Office of Scientific Research and Development in the early 1940s, Dryden became chief of a section developing a guided glide bomb. This section, located at the Bureau of Standards, was later expanded into the Navy Bureau of Ordnance Experimental Unit, with a staff of civilians from the Bureau of Standards and the Massachusetts Institute of Technology as well as officers and men of the U.S. Navy. The radar homing missile, BAT, which saw service in World War II in the Pacific, was designed by this team. The BAT missile destroyed many tons of Japanese shipping during the last year of the war. Fleet Air Wing One, under Rear Admiral John D. Price, used the BAT effectively against both ships and land targets. This was Dryden's first taste of the management of large projects, with which he would have so much to do later.

Dryden's wartime service was once described in his own words: "I headed an unusual group at the Bureau of Ordnance Experimental Unit which developed the radar homing missile, BAT. I also served as Deputy Director of the Army Air Force's Scientific Advisory Group headed by von Kármán. The group was appointed by General H. H. Arnold and many of us were

in Europe on V-E day in uniform with simulated rank to study the use of science by the various European countries." *Towards New Horizons*, the series of reports by the von Kármán group, proved invaluable in future years.

Dryden advanced steadily in the Bureau's organization, becoming Assistant Director in January of 1946 and Associate Director a few months later. Dryden and the late Dr. Lyman J. Briggs, Director of the Bureau, formed a friendship which lasted throughout their lives. There was an almost filial relationship between these two eminent scientists that will always be a treasured memory of their surviving families. When Dr. Briggs died, Dryden, a licensed local preacher of the Methodist Church, conducted his funeral services—a last tribute to his old friend.

PERIOD OF RESEARCH DIRECTION

In September 1947 Dryden transferred from the National Bureau of Standards to become Director of Research of the National Advisory Committee for Aeronautics (NACA). In 1949 he became Director of NACA, its senior full-time officer. He directed from Washington the activities of the Langley, Lewis, and Ames laboratories and the flight research stations at Edwards Air Force Base, California, and Wallops Island, Virginia. The magnitude of this responsibility grew to embrace, during the last year of NACA's existence, 8,000 employees and an annual budget of about \$100 million. Under his leadership, NACA produced a vast body of new knowledge which made possible routine supersonic flight and laid much of the technological groundwork for space flight that was to come. We discern here, perhaps as much as in any other place, the impact of Dryden's leadership. The development of high-speed wind tunnels, flight testing, and a companion competence for theoretical research within NACA contributed substantially to the leadership of the United States in supersonic flight.

In 1954 Dryden became the Chairman of the Air Force—

Navy-NACA Research Airplane Committee formed to guide the development of an airplane to explore the problems of flight at the highest speeds and altitudes then feasible. The series of experimental aircraft, beginning with the X-1, X-2, X-3, D-558, and culminating with the X-15, are well known. Some of these aircraft were developed and tested prior to 1954; however, the hypersonic research airplane, the X-15, drawing on the previous flight experience, was from its conception the concern of this Committee. Before he died, Dryden saw the X-15 reach a maximum speed in excess of 4,000 miles per hour and an altitude of nearly seventy miles. It had been Dryden who carried the X-15 program through the political labyrinth of Washington where funds for basic research and development were not plentiful. Much of this technology of manned flight came to bear in Project Mercury.

It was during this period that Dryden pressed for a solution to the critical reentry heating problem. This solution, based on knowledge accumulated in research, made it possible for the United States to proceed with assurance in the development of its ICBM program and manned satellites.

As we study Dryden's publications, we find a transition from descriptions of his own research to broader discussions of research policies. His Wilbur Wright Memorial Lecture, for example, in April 1949, read in London and entitled "The Aeronautical Research Scene—Goals, Methods, and Accomplishments," sketched his own interpretation of aeronautical research directions and results. Here he made perhaps one of his most important observations. In his own words: "It [research] should not in its entirety be limited to exploratory research or to coordinated theoretical and experimental work on experimental situations where complete understanding of basic phenomena is the principal goal. The needs of designers for systematic surveys of various areas and for research in support of development must be recognized and promoted by frequent and close

contact between designers and research workers. The selection of some common advanced technical development as the goal of both groups has proved to be an excellent means of promoting cooperation and of channeling research into directions permitting early application, without sacrificing the values inherent in the personal enthusiasm, initiative, and freedom of the research worker." This concept has lived and grown, and represents a fundamental policy of management in the conduct today of the programs and projects of NASA.

The days of closeness to the details of research were passing. Dryden would occasionally write invited summary papers in his own field of current importance. In 1956, for example, in collaboration with Duberg, he presented a paper at the Fifth General Assembly of AGARD in Ottawa, entitled "Aeroelastic Effects of Aerodynamic Heating." In the paper he pointed out by examples the degradations which could be expected in the aeroelastic properties of lifting surfaces at high speeds.

He sustained a continuous interest in applied mechanics. He served as president of the International Union of Theoretical and Applied Mechanics and as a member of the International Committee for the International Congress of Applied Mechanics. He took an active role in the organization of the Sixth International Congress for Applied Mechanics in Paris in 1946 and again at the Seventh International Congress in Istanbul in 1952. Together with von Kármán, he was an editor of *Applied Mechanics Reviews*. Contributions by Dryden may be found in the Proceedings of the 3rd, 4th, and 5th International Congresses of Applied Mechanics. He contributed to *Advances in Applied Mechanics*, Vol. I, 1948, with authorship of the section entitled "Recent Advances in the Mechanics of Boundary Layer Flow."

PERIOD OF PUBLIC POLICY

The final period in Dryden's life commenced dramatically in October 1957 with the launching of Sputnik I. The Executive

Branch and the Congress prepared immediately to establish a civilian agency to conduct explorations of space for peaceful purposes. With Dryden's help at critical moments, NACA was selected as the central building block of the new agency and he participated in the drafting of the legislation and its defense before the Congress. On August 8, 1958, President Eisenhower appointed Dryden as Deputy Administrator of the new agency, a position he held under three Presidents until his death.

Project Mercury was conceived and organized with Dryden playing a major role. Later, Dryden participated in the important planning for the Gemini and Apollo projects. His hand was prominent in the studies and recommendations that led to the decision to mount a lunar exploration mission. His commitment to the Apollo mission was demonstrated in a notable letter dated June 22, 1961, to the late Senator Robert S. Kerr, then Chairman of the Senate Committee on Aeronautical and Space Sciences. Dryden said in part: "The setting of the difficult goal of landing a man on the Moon and return to Earth has the highly important role of accelerating the development of space science and technology, motivating the scientists and engineers who are engaged in this effort to move forward with urgency, and integrating their efforts in a way that cannot be accomplished by a disconnected series of research investigations in the several fields. It is important to realize, however, that the real values and purposes are not in the mere accomplishment of man setting foot on the moon, but rather in the great cooperative national effort in the development of science and technology which is stimulated by this goal. . . . The national enterprise involved in the goal of manned lunar landing and return within the decade is an activity with critical impact on the future of this nation as an industrial and military power, and as a leader of a free world." Had Senator Kerr heard the Wilbur Wright

Memorial Lecture of 1949, he would have perceived a remarkable thread of uniformity in Dryden's approach to widely separated problems, a thread which dominated his thinking and will most certainly dominate national planning in science and technology for years to come.

In the last month of his life Dryden delivered the Thurston Lecture before the American Society of Mechanical Engineers. He pointed out that men had been engineers for thousands of years before the basic concepts of science were known. Engineers now follow the scientists' step-by-step approach to develop the technology from which real benefits can arise. But Dryden had a keen sense of social responsibility in planning engineering programs. He made the difficult choice among the many possibilities available to change the state of the art. In his Thurston Lecture he explained that the space program was already having an impact on engineering because of new requirements in weight, size, performance, and reliability under extreme environmental conditions.

As Director of the National Advisory Committee for Aeronautics for ten years, Dryden had great success in leading scientific and engineering research into important technical applications. When NACA was abolished in 1958 and the National Aeronautics and Space Administration (NASA) was set up by the Congress in response to Sputnik, Dryden was proposed by senior NACA members to be the Administrator of the new agency. He was seriously considered by the White House. However, Dryden's professional integrity may have antagonized members of the House Select Space Committee when he objected to an untested crash program to put a man on top of a missile in a suborbital space flight for propaganda purposes. He said this would have no more value "than shooting a woman out of a cannon at a circus."

The first NASA Administrator, President T. Keith Glennan

of Case Institute, insisted that Dryden be the Deputy Administrator and be overseer of all scientific and technical aspects of space research. In September 1958 Dryden was offered a distinguished professorship at M.I.T. In response he wrote: "When it became apparent that I would not be offered the position as head of NASA, I gave much thought to my future course of action. I decided that regardless of the action taken on appointments I would remain as long as I thought I could serve the national interest and ease the transition period for my associates of the last decade. Up to the present time I have been much encouraged by the attitude and actions of Dr. Glennan. He has successfully resisted great political pressure to make a certain appointment."

Dryden felt a special responsibility for the 8,000 civil service employees of NACA who were to be taken over by NASA. These people had been led, supported, chastised, or promoted and, in many instances, recruited by Dryden. Also Dryden carried over to NASA a most cordial and constructive relation with the military services, government regulating bodies, the universities, the air transport and manufacturing industries, and professional societies and research establishments. This was to prove invaluable to the U.S. space program.

Dryden's leadership capability itself was questioned in the report to President-Elect John F. Kennedy, January 12, 1961, by an *ad hoc* Advisory Committee on Space headed by Dr. Jerome Wiesner. This Committee found "a number of organizational and management deficiencies as well as problems of staffing and direction which should receive prompt attention. These include serious problems within NASA, within the military establishment, and at the executive and other policy-making levels of government." Specifically, the Wiesner Committee complained about the independent space programs of NASA and the military services with alleged overlap and duplication. The

Committee proposed central control in the Department of Defense of the Ballistic Missile Program, including "development of the missiles and associated control systems, base construction and missile procurement," and observed that "an adequate deterrent force is much more important for the nation's security than are most of the space objectives." It directly challenged the technical integrity of Project Mercury.

The Committee's concern for "the NASA preoccupation with the development of in-house research" and allegations that "space developments have all but halted any advance in the theory and technology of aerodynamic flight . . . and supersonic commercial aircraft" could be interpreted as pointing at Dryden's judgment. The Committee suggested that the Space Council might place the latter problem with another agency or make nongovernmental arrangements.

The Wiesner Committee recommended for NASA several "requirements that must be met." These were, in fact, outstanding features of Dryden's leadership. For example, "wide participation by scientists from universities and industrial laboratories. As the Home Secretary of the National Academy of Sciences, Dryden had close relations with the Academy's members and in particular with the Space Science Board that was established within the Academy to advise and assist NASA. "Exert the greatest wisdom and foresight in the selection of scientific missions and of the scientists assigned." This was one of Dryden's main concerns within the policy and budget limitations of the President and the Congress as the new NASA program gained direction and momentum.

The *ad hoc* Committee's report to President Kennedy, released to the press, did Dryden no harm. Probably it helped clear the air by requiring the new Administration to assess fully the space effort underway. When James E. Webb was asked by the White House to be the second NASA Administrator, he

accepted upon the condition that Dryden remain as Deputy Administrator.

Dryden had a leading role in the sphere of international cooperation. In 1959 he was appointed to assist Ambassador Henry Cabot Lodge at the first meeting of the United Nations Committee on the Peaceful Uses of Outer Space. His activities were largely responsible for a proposal by NASA, in December of that year, for joint research with other nations to promote international space cooperation. In the years that followed, after an exchange of correspondence between Premier Khrushchev and President Kennedy, Dryden was appointed by President Kennedy as the nation's chief negotiator for peaceful space cooperation with the Soviet Union. He engaged in a continuing dialogue with Academician Anatole Blagonravov on the possibility of such cooperation; from these talks came agreements for limited, but nonetheless real, cooperation between the two countries, particularly in the fields of meteorology and communications. Dryden carefully insisted upon clear mutual exchange in these beginning steps.

Working toward international cooperation and peace fitted with Dryden's philosophy. A man of sincere religious faith, he was a licensed preacher for the Calvary Methodist Church in Washington during most of his adult life. He had found the bridge between science and religion.

In 1962 Dryden was named by the Methodist Union as the Methodist Layman of the Year. The Rev. T. R. Bowen of his church pronounced the citation containing these words:

"At the very top of his profession, yet humble, self-effacing, and accessible; engaged in events and activities of far-reaching importance and world significance . . . but all the while, devout Christian and dedicated churchman, as deeply sensitive to the fact of value as he is to the value of fact; as alert to the moral and spiritual perils and possibilities of applied intelligence as

he is to the methods and data involved in his own specific discipline."

In an address at the Cosmos Club in 1954, Dryden said: "I am not one of those few who believe that we can abolish the use of force in the world. Policemen are still necessary and they must sometimes use force. As a nation we find it necessary to build great military power. I am confident that such strength is a greater contribution to the peace of the world at the present moment than military weakness." Dryden served a term as President of the Cosmos Club and was also a Life Trustee of the National Geographic Society.

Dryden was elected to the National Academy of Sciences in 1944. He served as the Chairman of its Section on Engineering from 1947 to 1950. In 1955 he was elected Home Secretary, a position he held until his death in 1965.

Dryden lived under a sentence of death after October 1961, when exploratory surgery disclosed a serious malignancy. Yet he continued on duty in spite of frequent hospital treatments. He conceded little to the illness that marked the last years of his life.

In a sermon he once said: "One major mark of rank in the organic world is the capacity to suffer. The aim of life, therefore, is not to abolish suffering, for that would abolish sensitivity, but to eliminate its cruel, barbarous, and useless forms. To willingly accept toil, trouble, and suffering, these are goals for scientists as well as for other men."

President Lyndon B. Johnson expressed the esteem of the nation when he said: "No soldier ever performed his duty with more bravery and no statesman ever charted new courses with more dedication than Hugh Dryden. Whenever the first American space man sets foot on the moon or finds a new trail to a new star, he will know that Hugh Dryden was one of those who gave him knowledge and illumination."

Dryden was always in advance of his fellows. He avoided the usual handicap of the precocious youngster. Instead of showing up his colleagues by his own quick wits, he consistently helped them to find solutions to their own problems. At the Bureau of Standards in the 1920s he helped Dr. Briggs determine the aerodynamic effects of the high-tip speed of propellers for the more powerful engines being introduced. He helped his Hopkins professor, Dr. Joseph S. Ames, when the latter was Chairman of NACA, as he did all of Dr. Ames's successors.

During World War II, Dryden supported the work of Dr. Vannevar Bush's National Defense Research Committee, most notably on the development of a radar-homing missile. As Scientific Deputy, he also assisted General H. H. Arnold's Scientific Advisory Group led by Dr. Theodore von Kármán. Everyone who came in personal contact with Dryden came to know and to appreciate his wisdom and help.

After Dryden's transfer to NACA in 1947, he assumed leadership in the fundamental research effort in the field in which he had made basic contributions twenty-five years before. It is fair to state that Dryden's 1920 work on supersonic aerodynamics led consistently to operational supersonic airplanes, the famous rocket-propelled X-15, and successful manned space flight. On February 10, 1966, the President of the United States presented to Mrs. Dryden the National Medal of Science awarded posthumously to Dr. Dryden: "For contributions, as an engineer, administrator, and civil servant for one-half century, to aeronautics and astronautics which have immeasurably supported the Nation's preeminence in space."

Hugh L. Dryden's life was given to helping good men get good results.

HONORS AND DISTINCTIONS

PROFESSIONAL AFFILIATIONS

National Academy of Sciences, Member and Home Secretary
Académie des Sciences of the Institut de France, Corresponding
Member and Foreign Associate Member
American Philosophical Society, Member
American Academy of Arts and Sciences, Fellow
Washington Academy of Sciences, Member
Philosophical Society of Washington, Member
American Institute of Aeronautics and Astronautics, Honorary Fellow, former President
Royal Aeronautical Society, Honorary Fellow
British Interplanetary Society, Honorary Fellow
Canadian Aeronautics and Space Institute, Honorary Fellow
American Physical Society, Fellow
American Society of Mechanical Engineers, Fellow
National Geographic Society, Life Trustee
National Academy of Engineering, Charter Member
National Committee for Geophysical Year, Member

HONORS AND AWARDS

Wright Brothers Lecture of the Institute of the Aeronautical Sciences (1938)
Sylvanus Albert Reed Award of the I.A.S. (1940)
U.S. Medal of Freedom (1946)
Presidential Certificate of Merit (1948)
Order of the British Empire (civil division) (1948)
37th Wilbur Wright Memorial Lecture of the Royal Aeronautical Society (1949)
Daniel Guggenheim Medal (1950)
Wright Brothers Memorial Trophy (1955)
Ludwig Prandtl Memorial Lecture of the Wissenschaftliche Gesellschaft für Luftfahrt (1958)
Career Service Award of the National Civil Service League (1958)
Baltimore City College Hall of Fame (1958)
President's Award for Distinguished Federal Civilian Service (1960)

Elliott Cresson Medal of the Franklin Institute (1961)
First von Kármán Lecture, American Rocket Society (1962)
Langley Medal of the Smithsonian Institution (1962)
John Fritz Medal (1962)
Rockefeller Public Service Award (1962)
Goddard Memorial Trophy (1964)
National Medal of Science (1965)

HONORARY DEGREES

Polytechnic Institute of Brooklyn (Sc.D., 1949)
New York University (D.Eng., 1950)
Rensselaer Polytechnic Institute (D.Eng., 1951)
University of Pennsylvania (Sc.D., 1951)
Western Maryland College (ScD., 1951)
Johns Hopkins University (LL.D., 1953)
University of Maryland (D.Eng., 1955)
Adelphi College (LL.D., 1959)
South Dakota School of Mines and Technology (D.Eng., 1961)
Case Institute of Technology (Sc.D., 1961)
American University (L.H.D., 1962)
Northwestern University (Sc.D., 1963)
Politecnico Milano (M.E., 1964)
Worcester Polytechnic Institute (Sc.D., 1964)
Swiss Federal Institute of Technology (Sc.D., 1964)
Princeton University (Sc.D., 1965)

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KEY TO ABBREVIATIONS

- Aeron. Eng. Rev. = Aeronautical Engineering Review
AIBS Bull. = Bulletin of the American Institute of Biological Sciences
Appl. Mech. Rev. = Applied Mechanics Review
Bur. Stand. Res. Pap. = Bureau of Standards Research Papers; in 1934 changes to National Bureau of Standards Research Papers
Bur. Stand. Sci. Pap. = Bureau of Standards Scientific Papers; in 1934 changes to National Bureau of Standards Scientific Papers
J. Aeron. Sci. = Journal of the Aeronautical Sciences
J. Appl. Mech. = Journal of Applied Mechanics
J. Wash. Acad. Sci. = Journal of the Washington Academy of Sciences
Mech. Eng. = Mechanical Engineering
NACA Tech. Note = Technical Note of the National Advisory Committee for Aeronautics
NACA Tech. Rept. = Technical Report of the National Advisory Committee for Aeronautics
NASA EP = Educational Publications of the National Aeronautics and Space Administration
NASA SP = Special Publications of the National Aeronautics and Space Administration
NASA Tech. Memo. = Technical Memorandum of the National Aeronautics and Space Administration
Nat. Geog. Mag. = National Geographic Magazine
NUEA Spectator = Bulletin of the National University Extension Association
Pennsylvania State College Eng. Exp. Sta. Tech. Bull. = Pennsylvania State College Engineering Experimental Station Technical Bulletin
Phys. Today = Physics Today
Proc. Am. Phil. Soc. = Proceedings of the American Philosophical Society
Proc. Internat. Congr. Appl. Mech. = Proceedings of the _____ International Congress of Applied Mechanics
Proc. Nat. Acad. Sci. = Proceedings of the National Academy of Sciences
RTCA Paper = Radio Technical Commission for Aeronautics Paper
U.S. Dept. State Bull. = United States Department of State Bulletin
Z. Flugwiss. = Zeitschrift für Flugwissenschaften

1920

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1924

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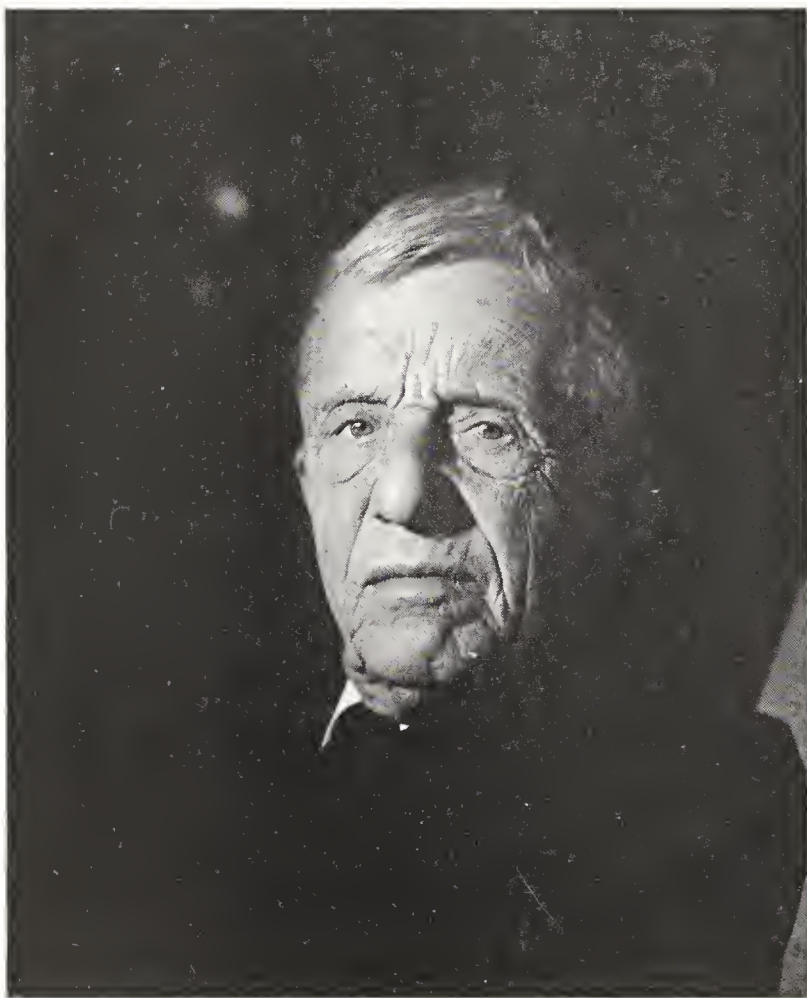
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L. P. Eisehart

LUTHER PFAHLER EISENHART

January 13, 1876–October 28, 1965

BY SOLOMON LEFSCHETZ

LUTHER PFAHLER EISENHART was born in York, Pennsylvania, to an old York family. He entered Gettysburg College in 1892. In his mid-junior and senior years he was the only student in mathematics, so his professor replaced classroom work by individual study of books plus reports. He graduated in 1896 and taught one year in the preparatory school of the college. He entered the Johns Hopkins Graduate School in 1897 and obtained his doctorate in 1900. In the fall of the latter year he began his career at Princeton as instructor in mathematics, remaining at Princeton up to his retirement in 1945. In 1905 he was appointed by Woodrow Wilson (no doubt on Dean Henry B. Fine's suggestion) as one of the distinguished preceptors with the rank of assistant professor. This was in accordance with the preceptorial plan which Wilson introduced to raise the educational tempo of Princeton. There followed a full professorship in 1909 and deanship of the Faculty in 1925, which was combined with the chairmanship of the Department of Mathematics as successor to Dean Fine in January 1930. In 1933 Eisenhart took over the deanship of the Graduate School. For several years Eisenhart was also executive officer of the American Philosophical Society. Eisenhart was survived by his second wife, the former Katharine Riely Schmidt of York, Pennsylvania

(his first wife, the former Anna Maria Dandridge Mitchell, died in 1913), by a son Churchill (by his first wife) of the National Bureau of Standards, by two daughters of the present Mrs. Eisenhart, and by six grandchildren. The following highly interesting and characteristically modest statement, left by Eisenhart, provides more complete information about his life:

"My parents, who were residents of York, Pennsylvania, had six sons, I being number two, born January 13, 1876. The only other survivor of the group is Martin Herbert, Chairman of the Board of the Bausch-Lomb Optical Company of Rochester.

"My father, after being a student at the York County Academy, taught in a country school until, acting upon the slogan by Horace Greeley—'Go West young man'—he went to Marshall, Michigan, and worked in a store. At the same time, he was apprentice to a local dentist. Being very expert with his hands, he soon acquired competence in the technique of dentistry. In due time he returned to York, set up a dentist office, and was married. He made sufficient income to meet the expenses of his growing family, but his intellect was too active to be satisfied by dentistry. Electricity appealed to him and he organized the Edison Electric Light Company in the early eighties. The telephone also made an appeal. He experimented with telephones and in the late nineties organized the York Telephone Company. Both of these companies are very active today.

"Mothers with such a large group of growing children would have felt that to provide for their well-being was enough. Not so with my mother. Not only did she do that to our satisfaction but she took part in our education before we went to school and helped in our 'home work.' In my case the result was that at the age of six years and five months I entered school and passed through the lower grades in three years instead of the normal six.

"We boys had a good time together. Each had his job to do in the family life, and when this was done we played baseball.

"While we were growing up my father was Secretary of the Sunday School of St. Paul's Lutheran Church and we all attended regularly. Also to an extent the social life of the family centered in the Church.

"Early in my junior year in the York High School the Principal, whom I recall as having been a very good teacher of mathematics, advised my father to have me withdraw and devote full time to the study of Latin and Greek, which I did in preparation for admission to Gettysburg College the next September (1892).

"In June 1893 I was awarded the freshman prize for excellence, greatly to my surprise, and the next year the prize in mathematics. By the middle of junior year, I was the only member of my class desiring to continue the study of mathematics. The professor, Henry B. Nixon, gave me books to study and report when I had any questions; there were no class sessions. The same plan on a more extensive basis was followed in senior year. This experience with the value of independent study led to my proposal in 1922 that the undergraduate curriculum for juniors and seniors in Princeton should provide for independent study, which was adopted and has continued.

"The next year, 1896, I taught mathematics in the preparatory school of the College and in October 1897 began graduate study at the Johns Hopkins. Professor Thomas Craig aroused my interest in differential geometry by his lecture and my readings in Darboux's treatises. Toward the close of 1900 I wrote a thesis in this field on a subject of my own choosing and in June the degree of Doctor of Philosophy was granted.

"In September 1900 my Princeton career began as Instructor in Mathematics, together with research and publication in the field of differential geometry. Five years later Woodrow Wilson instituted the Preceptorial System in Princeton with fifty Preceptors with the rank of Assistant Professor, to which group I was appointed. Four years later I was appointed Professor of

Mathematics. During 1925-1933 I was Dean of the Faculty and during 1933-1945 Dean of the Graduate School. Upon the death in 1928 of Henry Burchard Fine, who had been chairman of the department since 1902, I became Chairman and as such served until my retirement in 1945. Throughout the years I taught undergraduates and graduate students and conducted research in differential geometry, the results of which were published in books and journals."

EISENHART THE MAN

He was par excellence a family man and found in his family a great source of happiness and strength. Eisenhart was essentially a most modest man. The intimate atmosphere which surrounded him, its very serenity, was due in large measure to the care and devotion which he received from Mrs. Eisenhart. The Dean, as we all called him, did not seem to realize that he was an outstanding leader both in his field and in higher education. For outside his family he had two "loves": differential geometry (as research and study) and education. More about this below.

I remember my first association with Eisenhart at the 1911 summer meeting of the American Mathematical Society, then in its early years and still quite small. The meeting was held at Vassar College in an ordinary classroom, with hardly thirty participants. The usual dinner was a matter of perhaps fifteen participants. All those present were very young but included most of the coming leaders, among them Eisenhart. My close association with him dates from 1924 when I joined the Princeton faculty. One had already the firm impression of a most tolerant man, of the "live and let live" type, which I always found him to be.

EISENHART THE SCIENTIST

A true realization of what it meant to be a research scientist in the United States of 1900 requires a historical perspective

rare among the members of the younger generation. Our scientific advance is so rapid and so all-absorbing that time is simply not available for the necessary (and wholesome) glance backwards.

When Eisenhart began his scientific career we had only a handful of scientific centers worthy of note and perhaps half a dozen creative mathematicians. Only those with tremendous energy, stamina, and scientific devotion such as Eisenhart had in abundance forged in time their way to the front. Isolation was the rule. There were very few avid graduate and postgraduate students from whom to draw inspiration and fervor. Nothing resembling the wave of very capable undergraduates was in sight, and indeed Eisenhart never saw any sign of it. His very few mathematical colleagues were occupied with their own work, none of it in Eisenhart's direction. We are all aware of the general effect of this "splendid isolation."

Still another observation must be made. By modern standards the faculties of the day were all tiny, so that each faculty member had to carry a very full teaching load, much of which was of doubtful inspiration. This is what Eisenhart had to face, and did face unflinchingly, for many years.

Eisenhart's scientific devotion, stimulated by Thomas Craig of Hopkins, turned early and remained forever directed toward differential geometry.

In differential geometry Eisenhart appears in the direct line marked by Gauss, Riemann, and Eisenhart's immediate predecessors, Gaston Darboux and Luigi Bianchi. All the major differential geometers of the time up to and including Eisenhart lacked the mathematical panoply provided much later by the intense modern development of algebraic topology. Therefore, they came too early to attack the fundamental problem of differential geometry: to determine the exact nature *in the large* of a manifold from the knowledge of its differential properties *in the small*, that is, its local differential properties. It may be

said in parenthesis that even at this writing (1966) the problem has not been solved except in very special cases.

To return to Eisenhart, his life work was the study of the local differential properties of a manifold. As happens in many branches of mathematics, most of the problems that came up, and were accessible, were solved in time, leaving wide open the truly difficult core; as a consequence, research in the subject was discouraged, so that today its investigation is not assiduously pursued.

Eisenhart was certainly one of the most prolific investigators of the subject, as the large number of papers and books that he devoted to it shows. A chronological perusal of his books underscores his evolution, as well as that of differential geometry as a consequence of the intense intellectual excitement caused by the advent of relativity.

His first book, *A Treatise on the Differential Geometry of Curves and Surfaces* (Ginn and Company, 474 pp., 1909; 2d ed., Dover Publications, Inc., 1960), went deservedly through several printings. The treatment is gradual, thorough, and complete. It is one of the first high-grade mathematical books printed in the United States. It is excellent didactically and was well within reach of beginning graduate students of the time. The first chapter, about one tenth of the book, deals with the differential theory of curves in three-space. Although strictly introductory, it brings forward as early as possible some of the processes occurring later. The representation is parametric:

$$x_i = f_i(t), \quad i = 1, 2, 3$$

where the f_i are continuous in some range and differentiable as far as may be required, or for that matter even analytical. Tangents, osculating planes, curvature, and torsion are carefully described. The first chapter is an all-important preparation to the rest of the book. The proper topic of the book begins with

the second chapter. Surfaces are considered as subsets of Euclidean three-space R^3 given by a parametric representation

$$S: x_i = f_i(u, v), \quad i = 1, 2, 3$$

in terms of which all the important characters are defined.

The successive chapter headings give a good idea of the scope of the work: I. Curves in space (= three-space). II. Curvilinear coordinates on a surface. Envelopes. III. Linear elements on a surface. Differential parameters. Conformal representation. IV. Geometry of a surface in the neighborhood of a point. V. Fundamental equations. The moving trihedral. VI. Systems of curves. Geodesics. VII. Quadrics. Ruled surfaces. Minimal surfaces. VIII. Surfaces of constant total curvature. W surfaces. Surfaces with plane or spherical lines of curvature. IX. Deformation of surfaces. X. Deformation of surfaces. The method of Weingarten.

The last two chapters cover the topic which particularly interested Eisenhart and on which his thesis was based.

Eisenhart's next book, *Transformation of Surfaces* (379 pp., 1923), appeared fourteen years later (first edition, Princeton University Press; second edition, largely a reproduction, Chelsea Publishing Co., 1962). This is deeply a research book, the last one from the prerelativity era by our author. It still deals with surfaces in R^3 . Briefly speaking, most of the transformations that appeared in the preceding twenty-five years belonged to two types. They are systematically studied in the book, the aim being to bring some order in the related investigations. (The delay in the appearance of this book was due to World War I.)

We come now to the explosion caused in differential geometry by the appearance of Einstein's theory of general relativity. From the pre-Einsteinian, that is Riemannian, standpoint the universe as a three-dimensional manifold governed by a positive definite form:

$$ds^2 = \sum g_{ij} du_i du_j; \quad i, j = 1, 2, 3.$$

Special relativity, and especially the 1918 model—general relativity—upset these Newtonian notions, replacing the above ds^2 by one reducible to the type

$$ds^2 = dt^2 - dx^2 - dy^2 - dz^2$$

with light lines of zero length among other things. It so happened that the suitable mechanism for the new approach was already in existence, having been developed by Ricci as early as 1900. The day of tensor calculus, of the study of non-Riemannian geometry, had arrived, and Eisenhart was thoroughly prepared for it. This innovation affected all his later work and again is clearly evidenced in his later books. There are four of them and we shall say a few words about each.

Riemannian Geometry (first edition, Princeton University Press, 251 pp. [index and bibliography not included], 1926; second edition, 1949, 306 pp., 35 pages of new appendices, a most extensive historical bibliography going back to Riemann's fundamental paper of 1854). This is a treatment of the general manifold possessing a form

$$ds^2 = \epsilon \sum g_{jk} dx^j dx^k.$$

The important assumption made is that the sum is not necessarily positive definite, and $\epsilon = \pm 1$ is to guarantee $ds^2 > 0$ wherever the sum is < 0 . This is suggested primarily by the ds^2 of relativity. Without going into details let me note that the well-known Levi-Civita parallelism is introduced early (in Chapter II, the first chapter being devoted to an introduction to tensor analysis which is used throughout the book). The extension to non-Riemannian manifolds is reserved.

Non-Riemannian Geometry (American Mathematical Society Colloquium Publications, viii + 184 pp., 1927). This volume deals basically with manifolds dominated by the geometry of paths developed by Eisenhart and Veblen, as well as being

closely related to one due to Èlie Cartan. In Riemannian geometry parallelism is determined geometrically by this property: along a geodesic, vectors are parallel if they make the same angle with the tangents. In non-Riemannian geometry the Levi-Civita parallelism imposed *a priori* is replaced by a determination by arbitrary functions (affine connections). The main consequences of the deviation are investigated in the volume.

Continuous Groups of Transformations (Princeton University Press, 301 pp., 1933; Dover reprint, 1961). This represents a large excursion into a closely related but independent field, going back almost a century to Sophus Lie. Tensor calculus and recent Riemannian geometry are systematically applied in this major field.

An Introduction to Differential Geometry, with Use of the Tensor Calculus (Princeton Mathematical Series, 304 pp., 1940). As the title clearly indicates, this volume applies as many of the modern developments as possible, above all tensor calculus, to a number of the topics fully developed in the 1909 treatise of the author.

The last volume, which appeared five years before the author's retirement, underscores as strongly as possible his deep attachment to mathematical research and to the teaching of mathematics at the advanced level. During this very time Eisenhart was also fully occupied by his arduous administrative duties as Chairman of the Department of Mathematics and Dean of the Princeton Graduate School.

EISENHART THE EDUCATOR

In education as in much else, Eisenhart was and remained a man of his time, an American of the beginning of the century, a citizen of a nation so fabulously rich that it could afford all sorts of luxuries, including Eisenhart's educational credo that education was something not merely for the gifted but also for

the middle intellectual level. In less well-endowed nations economically—Europe by and large—this credo was untenable. Curiously Europe is in the process of veering in our direction while we may be moving in the opposite direction.

As it happens, Eisenhart's very moderate educational point of view enabled him to put through an important educational reform where more impetuous characters would certainly have failed. I refer to the famous "four-course plan" adopted by Princeton in the early twenties. It is of interest to dwell for a moment upon this event.

Doubtless as a consequence of the post-World War I era, unrest had developed at Princeton in regard to the undergraduate educational process in general. A committee was appointed to see what could be done. The report came back with the suggestion, promoted by Eisenhart, that a *four-course plan of study* be adopted for the junior and senior years. This was done; its operation began in 1923 and continues to this day. The general idea was to concentrate a student's work on his major field. This was accomplished by replacing the earlier five-course scheme by a four-course one, two of the courses to be in the student's major field. This went along with independent reading in a subject of the student's choice within his field and the writing of a serious terminal thesis with some research flavor. At the present time the level of students and their theses has risen exceptionally high, with resulting augmented work for the faculty! If I am not mistaken, this or an analogous scheme has been adopted in many places, but the originator of it is Eisenhart.

CONCLUSION

It is evident from all that has been said that Eisenhart was endowed with enormous energy: besides his considerable teaching load, he carried for years a full load as university administrator and as research scientist. During his deanship of the

Graduate School he was the *de facto* adviser to President Dodd on all scientific matters and was also chairman of the famous Research Committee of Princeton University—this at a time, 1930-1945, when there were very few such serious committees in the country.

HONORS AND DISTINCTIONS

HONORS

Sc.D., Gettysburg College, 1921; Columbia University, 1931; University of Pennsylvania, 1933; Lehigh University, 1935; Princeton University, 1952

L.L.D., Gettysburg College, 1926; Duke University, 1940; Johns Hopkins University, 1953

Decorated Officer of the Order of the Crown of Belgium, 1937

Luther Pfahler Eisenhart Arch, Princeton University, named by the Board of Trustees of Princeton University, 1951

PROFESSIONAL SOCIETIES

American Association for the Advancement of Science (Vice President and Chairman of Section A, Mathematics, 1916-1917)

American Mathematical Society (Vice President, 1913-1914; President, 1931-1932)

American Philosophical Society (Executive Officer, 1942-1959)

Association of American Colleges (President, 1930)

National Academy of Sciences, 1922 (Chairman, Section of Mathematics, 1931-1934; Vice President, 1945-1949)

National Research Council (Chairman, NRC Division of Physical Sciences, 1937-1946; Chairman, Advisory Committee on Scientific Publications, a joint committee with the NAS, 1940-1947)

Phi Beta Kappa

EDITORSHIPS

The Annals of Mathematics, Editor, 1911-1925

Transactions of the American Mathematical Society, Editor, 1917-1923

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KEY TO ABBREVIATIONS

- Accad. Lincei. Rendiconti = Rendiconti della Reale Accademia dei Lincei
 Am. J. Math. = American Journal of Mathematics
 Annali di Mat. = Annali di Matematica Pura ed Applicata
 Ann. Math. = Annals of Mathematics
 Bull. Am. Math. Soc. = Bulletin of the American Mathematical Society
 Circolo Mat. di Palermo. Rendiconti = Circolo Matematico di Palermo.
 Rendiconti
 Phys. Rev. = Physical Review
 Proc. Nat. Acad. Sci. = Proceedings of the National Academy of Sciences
 Trans. Am. Math. Soc. = Transactions of the American Mathematical
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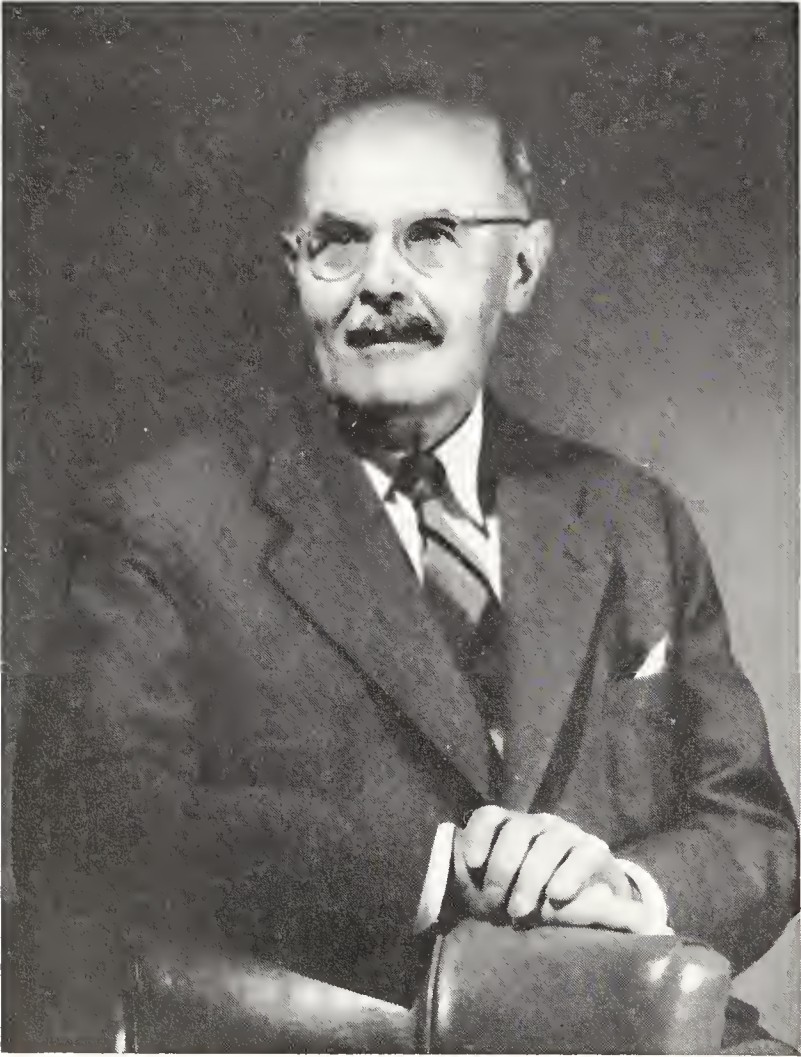
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Hermann O. Z. Finkler.

HERMANN OTTO LAURENZ FISCHER

December 16, 1888–March 9, 1960

BY W. M. STANLEY AND W. Z. HASSID

HERMANN OTTO LAURENZ FISCHER was born on December 16, 1888, in the university town of Würzburg in Bavaria and died on March 9, 1960, in the University of California Hospital in Los Angeles. He was the first of three sons of Emil and Agnes Fischer and was early exposed to academic surroundings since his father was Professor of Chemistry at the University of Würzburg. In 1892 the Fischer family moved to Berlin where Fischer's father, not yet forty years of age, succeeded to the chair of August Wilhelm von Hofmann and directed the activities of the Chemical Institute of Berlin University. Three years later Fischer's mother died and the three boys grew up in the rather lonely household of their widowed father who was greatly preoccupied with his teaching and research. However, Fischer remembered with pleasure the very informative conversations which the boys had with their father at lunchtime. During this period the Fischer boys went to the local *Gymnasium* where, Fischer recalls, "Latin, Greek, history and German were taught very well, mathematics somewhat and science very little." Fischer was given full freedom to select a profession but, of course, his early and continuing contacts with academic life and chemical research were quite influential. He retained early memories of a two-week visit within the Fischer household of Sir William Ram-

say, who told about the discovery of the noble gases—argon, helium, krypton, and neon. Fischer decided to become a chemist and his father, “conscious of the international aspect of science and deeply impressed with the philosophy of the English-speaking people,” sent him in 1907 to Cambridge University in England for a year. He then returned to Germany where he spent a year in military service in the little town of Lüneburg. With his military service completed Fischer returned to the study of chemistry, first in Berlin and then in Jena, where he started his doctoral program under the direction of the eminent Professor Ludwig Knorr at the University of Jena.

At that time Knorr and his students had succeeded in separating the pure keto form of ethyl acetoacetate by crystallization from alcohol, ether, or petroleum ether at -78° . The enol form had also been obtained by regenerating it from the sodium salt at low temperature. Fischer was able to apply somewhat similar methods to acetylacetone (2, 4-pentanedione) to obtain (*a*) the pure enolic tautomer, by direct crystallization at low temperature, and (*b*) a preparation rich in the ketonic modification. After receiving his doctor’s degree in 1912 with a thesis entitled “Zur Kenntnis des Acetylacetones,” Hermann Fischer decided to return to the Chemical Institute of Berlin University to continue his research studies with his father.

Under the direction of Emil Fischer the Chemical Institute was now one of the greatest scientific centers in Europe. There were experts in almost all fields of chemistry including such people as Diels, Franz Fischer, Gabriel, Otto Hahn, Lise Meitner, Leuchs, Wilhelm Traube, and Tiede. It was not surprising, therefore, that the Chemical Institute had a great attraction for the still somewhat lonely young chemist with his newly acquired doctor’s degree. Here he rapidly laid the foundation of what was to represent his major interest for the remainder of his career, namely, synthetic organic chemistry with emphasis on compounds of biological interest.

Emil Fischer at this time was primarily interested in studying the structure and chemistry of the natural tannins, and his son Hermann was assigned to the problem of synthesizing certain of the naturally occurring depsides. In this endeavor, the carbomethoxy (methoxycarbonyl) group, which had been used so successfully in the earlier polypeptide synthesis by Emil Fischer, was used to protect the phenolic groups of hydroxyaromatic acids during their conversion to the acid chlorides. The latter were then employed for esterifying other hydroxyaromatic molecules to produce the didepsides. Hermann Fischer was thus able to prepare the *ortho*- and *para*-diorsellinic acids. This study resulted in the publication of four papers bearing the names of Emil and H. O. L. Fischer in the *Berichte* in the issues of 1913-1914. This two-year period of scientific collaboration between father and son that Hermann Fischer called the happiest of his life ended with the catastrophe of World War I.

His two brothers had studied medicine and the three had planned to work as a team on problems presented by the living cell. But with the outbreak of war all three brothers were drafted into military service, Fischer in a chemical warfare unit and his brothers into military medical service. Both brothers died in the line of duty; hence when the war ended after four and one-half years, Fischer had to plan his life anew.

Science in Germany suffered an almost complete collapse for several years after World War I, owing to lack of personnel and facilities and to the pervading disorganized economic condition in the country. Meanwhile, the United States and England had been forced by the war to develop their own organic chemical industries, with auxiliary programs in basic research. During the war, Emil Fischer had kept the Chemical Institute in Berlin functioning as well as possible under the adverse circumstances. At this difficult period, at the end of 1918, Hermann Fischer returned to begin the work of redeveloping his career in chemistry. The shocking loss of his father in July 1919, coupled

with the general political and chaotic economic conditions of the times, made this task all the more difficult. However, in spite of many frustrations and difficulties, in the ensuing five years Hermann Fischer gradually gathered together a small research group and established a research program.

Two main lines of research were developed by Hermann Fischer during the period of 1920-1932 in the Chemical Institute. One was the study of the constitution and configuration of the naturally occurring quinic acid. The other dealt with the difficult chemistry of the trioses, glyceraldehyde and dihydroxyacetone, and related two-, three- and four-carbon compounds. The first publication on quinic acid, which appeared in 1921, described certain new derivatives of this tetrahydroxycyclohexanecarboxylic acid but left the position of one of the hydroxyl groups doubtful. With Dr. Gerda Dangschat, his chief assistant, he continued the investigation of this problem and in 1932 had finally established the correct structure and configuration of quinic acid. This result led in turn to the correct structure for chlorogenic acid, the quinic acid ester of caffeic acid, that occurs naturally in the coffee bean. Up to this time little was known of the chemistry of glyceraldehyde, dihydroxyacetone, glyoxal, glycolaldehyde, and pyruvaldehyde, and the efforts of the Fischer group did much to clarify this difficult field. The outstanding achievement of this work in Berlin was the successful preparation by Hermann Fischer and Erich Baer of DL-glyceraldehyde 3-phosphate. It was subsequently established in the laboratories of Otto Warburg, Gustaf Embden, and Otto Myerhof that the D-enantiomorph of this ester is the key three-carbon intermediate in alcoholic fermentation and glycolysis. Thus, the Fischer-Baer ester took its place along with the Harden-Young, Neuberg, and Robison esters in the Embden-Myerhof fermentation-glycolysis scheme.

In 1922 a happy event occurred in Fischer's personal life.

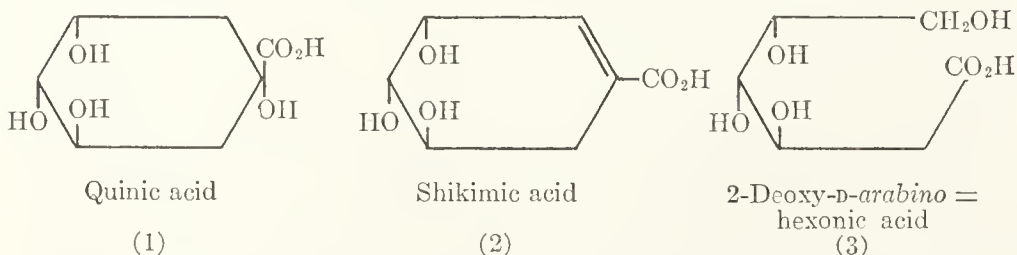
He married Ruth Seckels, in whom he found an understanding and devoted partner for life. This marriage resulted in three children, a daughter and two sons. About this time Fischer was appointed as Assistant Professor in the Institute, which was now under the direction of Professor Hermann Schlenk. This appointment gave him an opportunity to lecture to students and it was here that he acquired the ability that was to remain with him throughout his life to present really inspiring lectures. He remained at the Chemical Institute until 1932 when the threatening political situation in central Europe, coupled with an invitation to join the staff of the University of Basel, resulted in a move to Switzerland. The Fischer family found the Swiss university town of Basel a delightful place and were happy to leave the postwar turmoil of Germany.

The continuity of the triose research initiated in Berlin was assured when Erich Baer decided to accompany the Fischers to Basel, and their scientific collaboration produced many more outstanding results, both in Switzerland and, later, in Canada. Furthermore, although Gerda Dangschat remained in Berlin, she continued with Fischer their important work on the chemistry of the natural plant acids related to the inositols.

In pursuing the work on trioses, Fischer and Baer now developed a method of preparation for the enantiomorphous acetonated glyceraldehydes that was to lead later to their classical work on the asymmetric glycerides. The two collaborators succeeded in synthesizing the enantiomorphous acetonated glyceraldehydes from the mannitols. Inasmuch as the free glyceraldehyde could be readily obtained from these acetonated derivatives by mild acidic hydrolysis, the totally fermentable D-glyceraldehyde 3-phosphate and its mirror-image counterpart were made available for biochemical studies. The two investigators were able to show that the aldol reaction of unsubstituted D-glyceraldehyde with its ketonic isomer, dihydroxyacetone, leads in high yield

to D-fructose and D-sorbose, with practically a complete exclusion of the isomeric D-psicose and D-tagatose. Finally, the enantiomorphous acetonated glyceraldehydes provided, through reduction, the 2, 3-*O*-isopropylidene-D- and -L-glycerols, which opened up to Fischer and Baer, in their subsequent work in Toronto, the entire field of asymmetric glycerol derivatives.

During the Basel period the structural and configurational studies of quinic acid (1) by Fischer and Gerda Dangschat were extended to shikimic acid (2). This acid was first proved to be a 3, 4, 5-trihydroxycyclohexene-1-carboxylic acid by degrading it in a series of steps to both tricarballic acid and aconitic acid. The configurational relationship of the three hydroxyl groups was then clarified through consideration of the chemical behavior and optical properties of the δ -lactone of dihydroshikimic acid. Ultimately, in a most outstanding series of experiments, the parent shikimic acid was converted to 2-deoxy-D-arabino-hexonic acid (3). Thus, the configuration of the acid and the precise location of its double bond were determined, and its relationship to quinic acid and to D-glucose was established. Some fifteen years later, this precise work by Fischer and Dangschat served Bernard D. Davis as the basis for identification of shikimic acid as a key intermediate in the biological transformation of carbohydrates into aromatic amino acids. The decisive biochemical experiments were performed by Davis with a sample of the acid contributed by Fischer and Dangschat.



Unfortunately, the rumblings of war in Europe increased and by 1936 Fischer was convinced that war was coming again. His

two sons were approaching military age and Fischer was not happy about the prospect of their having to go off to war and to an uncertain future. So once again the political situation dictated a move, this time in 1937 to the Banting Institute of the University of Toronto in Canada. The previous year on a lecture trip to America Dr. and Mrs. Fischer had met Sir Frederick Banting and Dr. Charles Best, who were greatly impressed by the Fischers. Also on this trip the Fischers had their first experience with American hospitality at the hands of Dr. and Mrs. H. T. Clarke of Columbia University, Dr. and Mrs. H. B. Vickery of Yale University, and the great sugar chemist, Claude S. Hudson of Washington. The impressions gained by the Fischers during this trip were most influential, together with the political situation in Europe, in the decision to accept the offer from the University of Toronto. Fischer had anticipated the war by only two years, for World War II broke out in 1939. All during the war Fischer, despite his German origin, was able to continue his research work largely because of the very understanding attitude of the University of Toronto and the Canadian government and, of course, because of the actions of the Fischer family itself.

Dr. Erich Baer and a young Swiss chemist, Dr. Jean Grosheintz, followed Fischer to Canada. Later, another young Canadian chemist, John C. Sowden, joined them and with these able young men Fischer formed a small but stimulating and productive research group. The work of Fischer and Baer with enantiomorphous glyceraldehydes was now extended to the glyceride field. They succeeded in preparing the enantiomorphous α , β -diglycerides by benzylation of the acetonated glycerides, followed by hydrolysis of the acetone group, acylation and hydrogenolysis of the benzyl blocking group. Of considerable interest was the observation that, although the asymmetric α -mono- and α , β -di-glycerides showed measurable optical activities, no rota-

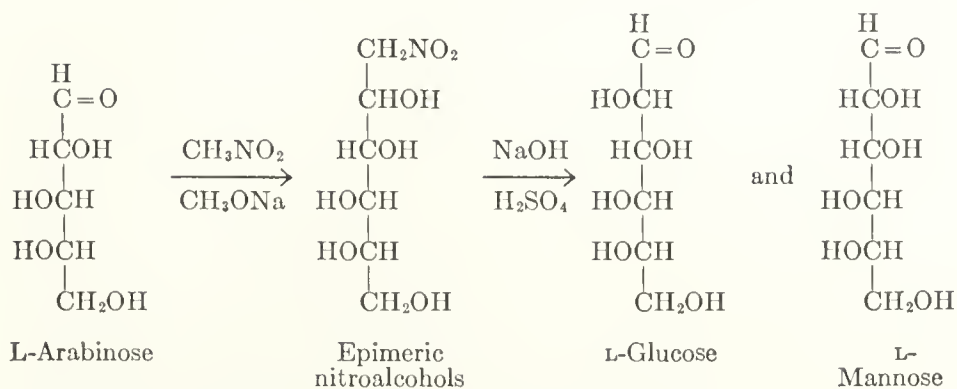
tion could be detected for any of the asymmetric, mixed triglycerides that contained only aliphatic ester groups. Thus, any failure to observe optical activity in natural fats does not necessarily mean that they are asymmetrical or racemic.

The enantiomorphous acetonated glycerols were also converted through phosphorylation, followed by hydrolysis of the acetone group, to the two α -glycerophosphoric acids (glycerol 1-phosphates). The L(—)- α -glycerophosphoric acid, obtained in this way from 2, 3-*O*-isopropylidene-D-glyceraldehyde, proved to be identical in every respect with the biological α -glycerophosphoric acid isolated from natural phosphatides and from fermentation and glycolysis media. Studies of the action of lipases on the enantiomorphous methylated α -monoglycerides, and of phosphatases on the two α -glycerophosphoric acids, demonstrated in each case that the enzymes act at markedly different rates on the two members of the antipodal pairs.

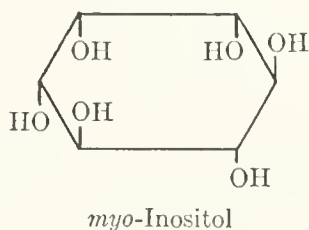
Other investigations by Fischer and Baer in Toronto included the study of the asymmetric glycerol ethers, the development of syntheses for dimeric glycerose 1, 3-diphosphate, glyoxal semiacetate, L(+)-propylene glycol, the β -phosphatidic acids, and *O*-phospho-enolpyruvic acid. In collaboration with Dr. Jean Grosheintz, they also established conditions for glycol-cleavage reactions with lead tetraacetate in partially aqueous media.

Then, in collaboration with Dr. Sowden, Fischer developed relatively simple conditions for the reaction of the sugars with nitromethane. For example, L-arabinose yielded L-glucose and L-mannose by way of the epimeric nitroalcohols, as shown in the accompanying formula. Thus, a new and general method of ascending the sugar series became available to supplement the classical cyanohydrin method of H. Kiliani and Emil Fischer.

An important adaptation of the aldose-nitromethane reaction was also made by Fischer and Jean Grosheintz. By addition of nitromethane to 1, 2-*O*-isopropylidene-D-xylo-pentodialdose and



subsequent hydrolysis of the acetone group, they obtained a mixture of 6-deoxy-6-nitro-D-glucose and 6-deoxy-6-nitro-L-idose. A second, intramolecular reaction of these substances led to a mixture of deoxynitroinositols and, by reduction, to the related aminodeoxyinositols. One of the latter was successfully deaminated a few years later by T. Posternak to *myo*-inositol, thus completing a synthesis that started from D-glucose.



Hermann Fischer's laboratory in Toronto attracted a number of able young chemists. Among the postdoctoral fellows were Dr. Henry Lardy, who worked out a practical method for the chemical synthesis of D-glucose 6-phosphate, and Beat Iselin, who developed the first synthesis of *myo*-inositol 2-phosphate.

In spite of the productive scientific period at Toronto and the scientific and social climate which he enjoyed, the cold winters left much to be desired, especially now that Fischer was approaching his sixtieth year and his wife's health was affected by the weather.

One of us came to know and to admire Fischer during this

period, so when, in addition to planning and building a Virus Laboratory at the University of California at Berkeley, President Sproul added the obligation of starting a new Department of Biochemistry, Dr. Stanley's thoughts naturally turned to Fischer. Here was a gentleman and a scholar of the first order who in his own early life and through his father represented a relationship with the very beginnings of biochemistry, who over the years despite great difficulties had accomplished very important advances in carbohydrate chemistry, and who could undoubtedly provide a perspective of the real place of biochemistry in a unique way. It was, therefore, a pleasure to invite him to join the staff as Professor of Biochemistry in the newly established Department of Biochemistry of the College of Letters and Science.

The period in Berkeley from 1948 to 1960 was described by Fischer shortly before his death as one of the happiest in his life. In the stimulating environment of the Berkeley campus, Fischer, with the aid of junior staff members and visiting post-doctoral fellows, now turned his attention mainly to the carbohydrates and inositols. In addition to lecturing and the direction of his research program, he also assumed important administrative duties as chairman of the Department of Biochemistry from 1953 until his official retirement in 1956.

With Dr. Donald L. MacDonald he succeeded in working out a new and efficient method for descending the sugar series. This method was based on the sugar diethyl dithioacetals (mercaptals), which, on oxidation with peroxy acids, readily produced derivatives giving the corresponding sulfones. Upon treatment of these derivatives with mild alkali they were degraded in good yield to the next lower sugar and bis(ethylsulfonyl) methane. This new method proved to be a valuable supplement to the older, classical degradation methods of Ruff, Wohl, and Weerman.

Another significant contribution to the chemistry of carbo-

hydrates was the development, with Dr. C. E. Ballou and Dr. D. L. MacDonald, of methods for the preparation of the dialdoses. Starting with di-acetonated D-inositol, they were able to obtain *manno*-hexodialdose. *xyl*o-Pentodialdose was produced by way of the disulfone to *myo*-inosose-2 and D-*gluco*-hexodialdose by use of the 6-deoxy-6-nitro-D-glucose.

Later Fischer, MacDonald, and Ballou prepared the biochemically important phosphorylated sugar D-erythrose 4-phosphate. The importance of this lower phosphorylated sugar was established when Srinivasan, Katagiri, and Sprinson demonstrated its condensation with *o*-phosphoenol-pyruvic acid to 5-dehydroshikimic acid by *Escherichia coli*.

In 1956 Fischer became Professor Emeritus, but he maintained an active and productive research laboratory at the Department of Biochemistry until the end of his life, early in 1960. During this period he became interested in the application of the aldehyde-nitromethane reaction, and particularly to its application for the synthesis of aminodeoxy sugars and aminodeoxy inositols. In collaboration with Dr. Hans H. Baer, making use of this reaction, he was able to synthesize 3-amino-3-deoxy sugars and, later with Dr. A. C. Richardson, a mixture of amino-anhydrodeoxy sugars. Working under the guidance of Fischer, Dr. F. W. Lichtenthaler obtained a mixture of 1,4-dideoxy-1,4-dinitroinositols, one of which, upon reduction, yielded *neo*-inosadamine-1,4.

On the occasion of the official dedication of the Biochemistry and Virus Laboratory building in 1952, which fortunately could be arranged to coincide with the centennial of the birth of Emil Fischer, Dr. Fischer donated his personal cherished possession, the private library of his father, to the Department of Biochemistry. This was done as a gesture of appreciation and good will toward the University. The library now bears the name of the Emil and Hermann Fischer Library.

Hermann Fischer was the recipient of many honors and

awards. In 1949 he received the Sugar Research Award for his contributions to carbohydrate chemistry. Election to the National Academy of Sciences (United States) came in 1954 and a year later he was presented with the Adolf von Baeyer Medal, one of the highest scientific honors in Germany. In 1958 he was recipient of the Hudson Award of the Division of Carbohydrate Chemistry of the American Chemical Society. This award was particularly fitting in view of his close personal and scientific relationship with the late Claude S. Hudson, after whom the award is named. Also in 1958, to mark his seventieth birthday and his many services to biochemistry, Fischer was honored with an anniversary issue of the *Archives of Biochemistry and Biophysics* to which many of the world's foremost biochemists contributed. The following year, in 1959, he was presented with an honorary Doctor of Science degree by the Justus Liebig University in Giessen, Germany. He was an elected Fellow of the New York Academy of Sciences and of the Chemical Institute of Canada, and a member of the Society of Biological Chemists, the American Chemical Society, the Chemical Society (London), the Swiss Chemical Society, and the American Association for the Advancement of Science.

Professor Hermann Fischer had a combination of exceptionally fine and rare human attributes. He possessed an innate kindness and sincere concern for the welfare of others. He had a zest for life, a sense of humor, and a great capacity for friendship which won him innumerable devoted friends and admirers throughout the world. He was a gentleman and a scholar of the first order.

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KEY TO ABBREVIATIONS

- Am. Chem. Soc. Meeting, Abstrs. = American Chemical Society Meeting, Abstracts
Ann. Rev. Biochem. = Annual Review of Biochemistry
Ber. = Berichte der deutschen chemischen Gesellschaft
Biochim. Biophys. Acta = Biochimica et Biophysica Acta
Can. J. Res. = Canadian Journal of Research
Helv. Chim. Acta = Helvetica Chimica Acta
J. Am. Chem. Soc. = Journal of the American Chemical Society
J. Biol. Chem. = Journal of Biological Chemistry
Naturwiss. = Die Naturwissenschaften

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Alexander Forbes

ALEXANDER FORBES

May 14, 1882–May 27, 1965

BY WALLACE O. FENN

ALEXANDER FORBES was born in Milton, Massachusetts, and died at the age of eighty-three in the same town. He was a distinguished member of a distinguished family. A great-grandfather, John Murray Forbes, was a trusted U.S. diplomat in South America, under President John Quincy Adams; and a grandfather by the same name had a business career in China, became a railroad magnate, built a cruiser for the Navy in the Civil War, helped to organize Negro regiments in Massachusetts, and was responsible for establishing the Forbes family on Naushon Island, off Woods Hole, Massachusetts, where they still live in the summer. His son, William Hathaway Forbes, was a Civil War hero who was captured by the Confederates but succeeded in effecting his escape. He became President of the Bell Telephone Company, and married Edith Emerson, the daughter of Ralph Waldo Emerson. The last and eighth child of this marriage was Alexander Forbes. An older brother was W. Cameron Forbes, who coached football at Harvard, became Governor-General of the Philippines and United States ambassador to Japan. Still another brother, Edward Forbes, was Professor of Fine Arts and Director of the Fogg Art Museum at Harvard.

Dr. Alexander Forbes married Charlotte I. Grinnell in

1910, and leaves a son, A. Irving Forbes of Exeter, New Hampshire, three daughters, Mrs. W. Andrew Locke of Milton, Mrs. Katherine F. Goodhue of Rockville, Maryland, Mrs. Joseph R. Frothingham of South Dartmouth, Massachusetts, and ten grandchildren.

It is interesting to record that Dr. Forbes met his future wife in the Adirondacks, at the Putnam Camp, founded by Henry P. Bowditch, the first Professor of Physiology at Harvard. They often visited this place in later years.

From his father, Alex probably inherited his love of the sea and his venturesome spirit, and from his mother, his warm and friendly disposition, his humanistic philosophy of life, and his liberal religious views. He once expressed his religious views in an article in the *Christian Register* (September 1951) and elsewhere he wrote: "Viewing the pageant of the universe in its entirety, and contemplating man's rise from the protozoan to his highest spiritual stature, I find in the creative force that did all this, something we can worship with all the reverence that is in us." He was a member of the Unitarian Church in Milton, and the funeral services held for him there were described as dignified and impressive, simple and natural, as befitted the man. Alex was every inch a gentleman, loyal to his many friends, considerate of others, quiet and self-controlled but confident and decisive in action. He always seemed to be in tune with his environment and always in a good humor.

A cousin of Dr. Forbes, the late Dr. John F. Perkins, Jr., characterized his relative as follows: "Love of living every minute of the day, and a sustained spirit of adventure were Cousin Alex's way of life. Yet, I realized after he died that I had never really understood how different he was from the confident, swashbuckling prototype of an adventurer on the one hand, and the absent-minded professor, which he somewhat resembled, on the other. His love of living was a love of simple

things, like the sea or the mountains, and his apparent absent-mindedness—the result of preoccupation with important matters—concealed a cool-headed clarity of thinking and acting which enabled him to fly a plane for over a thousand hours without a major mishap, and to sail his boats for tens of thousands of miles with very few groundings. In 1926, when he took up flying in earnest, at the age of forty-four, many of us feared it would be only a matter of time before he had a serious accident; yet he never did, even though in his late seventies he was flying to and from far-off physiology meetings alone in his plane. Thus, a gentle, reticent, and absent-minded manner concealed a strength of will and a calmness which came to the fore in difficult situations, such as when he was lost at the age of seventy-six, alone in his airplane, somewhere between Buffalo and Boston, but said, ‘I saw the top of Mount Monadnock sticking out of the fog and then I knew where I was.’ He never panicked in such difficult situations.”

Dr. Forbes had a fortunate boyhood with valuable opportunities for cultivating a love of adventure and manly sports, along with more scholarly pursuits. He attended Milton Academy from 1889 to 1899, where he received Honorable Mention three times for landscape and marine photography, helped to found a Debating Society, tied for third place in the Senior Class high jump, and read “a very witty class history” at Commencement. In writing of his school experiences he stated that he “liked especially [his] courses in physics and advanced Greek, both being taught by the brilliant and versatile scholar James Hattrick Lee.” Between school and college Alex took a year out “living partly on a cattle ranch in Wyoming, camping in the Big Horn Mountains, working for a time in an electro-chemical mill at Rumford Falls, Maine, then traveling in the Pacific Coast states, and during the summer, in Switzerland, France, Holland, England, and Scotland.”

In 1900 he entered Harvard College and graduated with an A.B. degree in 1904. As a student at Harvard he played on his class football team and was a member of several clubs, including the Institute of 1770, the Hasty Pudding Club, Signet, and Delta Phi. His graduation was followed by a year of graduate study with Professor G. H. Parker, in zoology, where he "learned the rudiments of electrophysiology." At that time this probably meant little more in the way of laboratory technique than the use of an inductorium with which to stimulate frog muscle, and the use of a smoked drum to record the contraction. Anyhow, this represented a nice combination of his interests in physics and biology. Thus he acquired an A.M. degree in 1905 and, more important, came to realize, on the advice of Professor Parker, that what he really wanted was a career devoted to the study of the physiology of the nervous system by the use of physical techniques.

Before starting on this career, however, he spent another year mostly in Wyoming, camping much of the time with his brother in a cabin which they had built during a previous summer at Lake Solitude, far up in the Big Horn Mountains. A period of outdoor living had been recommended by a physician in the hope that it might benefit a progressing otosclerotic defect. There they engaged in a combination of work outdoors, chopping wood and shooting elk, and indoors, studying chemistry and astronomy. An intriguing account of this experience is provided in an article which he wrote for the *Sheridan (Wyoming) Press*, July 18, 1962.

The next four years were spent at the Harvard Medical School where he received the M.D. degree in 1910. Before graduating he had become author or co-author of four scientific papers, two on foreign body giant cells, one on the Mendelian inheritance of hair patterns in guinea pigs, with Professor W. E. Castle, and one with Professor L. J. Henderson on "the esti-

mation of the intensity of acidity and alkalinity with dinitrohydroquinone." This in itself was an extraordinary accomplishment. In accordance with his preconceived plan, he did not take the usual internship, but immediately after graduation, joined the Department of Physiology, as instructor, with Professor W. B. Cannon. There he spent part of his time teaching physiology to medical students, and began his long series of papers on nervous reflexes. His research work was greatly enhanced by an opportunity to spend a year (1911-1912) in Liverpool with Professor Charles Sherrington, and a short time also with Keith Lucas at Cambridge. From these experiments he derived the real inspiration for his life work on conduction in reflex pathways. Later, in 1921, he spent a happy summer working with E. D. Adrian in Cambridge, England. During that summer, according to recent testimony of Lord Adrian, he made occasional trips to Oxford to consult with Sir Charles Sherrington and take lessons in flying.

Alex returned in 1912 from his first visit to England with new ideas, new techniques, and new equipment. He soon acquired one of the new Einthoven string galvanometers, the first to be installed in the Boston area. This instrument brought a new degree of accuracy into the timing of delays and interactions in the spinal reflex centers. The initial studies with it culminated in two classical papers with Alan Gregg in 1915 and other papers later, with other collaborators, in 1923 and 1928. Other studies during this period dealt with effects of anesthesia, of fatigue, of temperature changes, and of beta rays of radium on either nerve conduction, spinal reflex action, or the electromyogram. Usually the subject of study was the decerebrate cat, but sometimes it was man, and once it was an elephant which came as part of a circus troupe to entertain the children at the Children's Hospital, just outside Alex's laboratory window. He also obtained (with McPherson) what were

probably the first photographic records of spontaneous electrical activity of the exposed cerebral cortex of an animal, although, like his predecessors with more primitive instruments, he failed at the time to appreciate their full significance. Later, he turned to problems of spinal shock and muscle tonus and also to sensory systems, notably the proprioceptive and the auditory.

Improvements in recording technique were a continuing interest. Alex pioneered in the application of the electron-tube amplifier in the days when a condenser-coupled amplifier was a new idea, and he is credited with the first reported application of an electron-tube amplifier in nerve physiology (1920), using his string galvanometer as the recording instrument. Later he (with Renshaw) was one of the first to explore the brain with microelectrodes.

Probably the best-known and most influential of Alex's papers appeared early in this period (1922) in *Physiological Reviews*. In this classical work he endeavored to analyze in minute detail the transmission of impulses through reflex centers of the spinal cord and brain, and to interpret the observed reflex delays, summations, inhibitions, and after-actions in terms of what was known concerning the physiology of isolated nerves. In this effort he set a philosophy and a goal, both for himself and for a generation of neurophysiologists. It was not that his theoretical interpretations were all correct. Many of them in fact were soon proved inadequate, some of them by Alex himself. The point was that he demonstrated the possibility of an experimentally based, self-consistent system of interpretation, and he presented one which bristled with invitations for experimental tests and further exploration. It was a superb effort and established his reputation as a leader in the field. In the opinion (1949) of Hallowell Davis, who was his chief collaborator for many years, "This article can

justly be described as one of the foundations of the new science of cybernetics." This opinion was based on the great influence which this article had on Norbert Wiener and others who were interested in theoretical problems of organization and control.

The paper which Alex himself considered his most important contribution to science was published in 1926, with H. Davis, D. Brunswick, and Anne Hopkins as co-authors. This paper showed that, contrary to the teaching at that time, a nerve impulse in a uniformly narcotized length of nerve traveled with a uniform but subnormal electrical intensity and velocity, but without that progressive diminution which was the description previously taught. That description of "conduction with a decrement," Alex had reasoned quite correctly, was a violation of the all-or-none law of nerve conduction, and it was for the express purpose of clearing up this discrepancy that the investigation was first initiated.

Alex worked a great deal on problems of excitation and inhibition in the nervous system, and he finally accepted the idea that the transmission of excitation at synapses from one nerve cell to another might not be entirely electrical in nature but might involve the participation of excitatory or inhibitory neurohumors. These he whimsically described as "soup at the synapse," a term which came into wide use among neurophysiologists. Alex was always ready to change his published views when experimental evidence required it. He used to say, "People should understand that the literature is always in a state of flux."

In general it can be said that Forbes was one of the real pioneers in applying electrical recording to the central nervous system and in developing a unified interpretation, based directly on such measurements, which would include, alike, muscle, sense organs, peripheral nerve, and the reflex activities of the central nervous system.

This brief summary of the scientific work of Dr. Forbes shows that he made substantial contributions and had an important influence on his contemporaries. He published over a hundred scientific papers and had many distinguished collaborators. He was an instructor from 1911 to 1921, associate professor until 1936, and professor until he retired as Professor Emeritus in 1948, at the age of sixty-six. Even after his retirement, however, he continued his scientific work in the Department of Biology at Harvard. There he made a study of vision in the turtle, using an electroretinographic technique, and worked part time at the Veterans Hospital on the measurement of barbiturate narcosis. He continued to attend scientific meetings with his usual zest until the last year of his life, and often participated in the discussions. He became a familiar sight at such meetings, always in the front row, with his hearing aid and his head of white hair, bent forward to catch every word. It has been remarked that he had the features of his famous grandfather, Ralph Waldo Emerson, and he certainly had a distinguished and pleasant appearance. He usually went to meetings with his wife, Charlotte, and in his own airplane, although in his later years his license prohibited him from flying at night—somewhat to his chagrin. The writer and his wife have shared many Society banquet tables with Dr. and Mrs. Forbes; at future meetings the Forbeses will be sorely missed by many friends.

In 1910 Dr. Forbes was elected a member of the American Physiological Society and served as its Treasurer from 1927 to 1936. He was honored by election to the American Philosophical Society in 1931, and to the National Academy of Sciences in 1936. He received an honorary S.D. degree from Tufts College in 1952 and an honorary D.Sc. degree from Johns Hopkins in 1954.

In addition to his scientific work, Dr. Forbes loved outdoor

life, including skiing, horseback riding, figure skating, sailing, canoeing, camping, and flying. All of these he continued almost to the last with little or no concession to his advancing years.

To prolong his glide in a ski jump he once constructed a glider-type wing which he strapped to his shoulders before going over the jump. The experiment did not go exactly as planned, and he crash-landed below the jump in a jumble of broken spars and torn fabrics. As usual, he was more concerned over the failure of the experiment than he was with his own injuries which, fortunately, were minor.

With a canoe or kayak he made fifteen trips down the rushing water and the hemlock-lined gorges of the Westfield River, between 1914 and 1932, and recorded the experiences of his venturesome group in his "Twenty Years of the Westfield River," published in *Appalachia*, June 1937. Not infrequently the canoes capsized in the white water but this did not dampen the enthusiasm of the canoeists. Alex learned to know the river intimately, its danger spots and its favorable camp sites, and he recorded the height of the water nearly every year for twenty years.

Alex was particularly fond of sailing, and he did a great deal of cruising, mostly between Naushon Island and his cottage at Harbor Island, Maine; a beautiful but wild and rugged spot. He owned at various times the schooners *Black Duck* and *Ramah*, and the ketch *Stormsvala*. On one trip, in 1933, he sailed the *Ramah* all the way to Naples, Italy, with an all-male amateur crew, including his brother Edward and various relatives. He left the ship there for the winter and returned in the summer of 1934 with a crew of seventeen including his own family, Samuel Eliot Morison, and a group of college boys, to make a six weeks' cruise of the Mediterranean. Professor Morison wrote a delightful account of that voyage in his *Spring Tides* (Riverside Press, 1965). In that book he describes Alex

as "a truly remarkable character and one of the most versatile men of our era," and, in general, as "the best all-around shipmate and the most Christlike character I have ever known, whether layman or cleric."

There is ample testimony from many sources of Alex's skill as a navigator, and this entitled him to write a book, *Offshore Navigation in Its Simplest Form* (Eastern Science Supply Co., 1935). Morison writes that "as a practical navigator with sextant and dead reckoning, few professionals are his equal . . . his hardihood and indifference to creature comforts are the constant astonishment of his friends. Two years ago he sailed a small open lifeboat from Boston to Mt. Desert just for the fun of it. The young lieutenant commander who shared the same tarpaulin and the exiguous meals admitted that it was pretty rugged, but that Dr. Forbes' company made the cruise a wonderful experience."

Dr. John Perkins contributed the following: "Enthusiasm, the enthusiasm of a boy was ever present with Cousin Alex on the many cruises I was lucky enough to go on—to Nova Scotia in 1927, when we never stopped sailing and entering harbors in spite of fourteen consecutive days of fog; to Bermuda in 1930; to Naples from Cape Cod on the *Ramah* in 1933.

"Many of Cousin Alex's best physiological works were completed after these cruises had started. He would work away on the article in the cabin, as we sailed along with the schooner heeled way over, to be greeted by an occasional dash of salt water usually from a careless deck-swabbing operation, but he would manage to finish the paper nonetheless, in these less than ideal circumstances. As John Pappenheimer has indicated in his excellent Memorial Minute to Alexander Forbes, which was read in 1965 to the Faculty of the Harvard Medical School, he always kept a daily journal, kept up the log of the schooners *Black Duck* and *Ramah*, and the ketch *Stormsvala*, and the

log of the Harbor Island Club; possibly inheriting a sense of history from his grandfather Ralph Waldo Emerson. Literally hundreds of young and older people shared these happy adventures with Alexander Forbes. I was lucky enough also to receive a complete training in piloting and navigation from him as many others did."

In his 25th-anniversary report to the class of 1904 Alex writes of his cruises as follows: "In the matter of recreations I am overstocked, since a full count reveals that I have over fifteen hobbies. Most of my recreations depend upon water, frozen or liquid. The most absorbing interest, outside of family and work, is boating, especially cruising in sailboats; and I like that best when I can combine it with family by taking my children along and teaching them the lore of the sea. Everything pertaining to ships fascinates me, especially the theory and practice of navigation. This has led me to design a mechanical interpolator which aids me in the use of tables when working up a sight of the sun or a star These cruises mean much more than recreation; indeed they are often fatiguing and taxing to one's strength, for they have all been made without paid hands of any sort. They afford constant practice in self-reliance and decision, they give intimacy with the ocean and the weather—elemental things of nature; and they have given me some of the best friendships I have, for the men who make the best sailors are usually the kind that make the most durable friends."

For the summer of 1931 Alex had planned a cruise to Labrador, largely for recreation, but by chance Dr. Grenfell suggested that the coast of Labrador needed better charts. Intrigued with the idea of such a worthy objective for his cruise, Dr. Forbes consulted with the American Geographical Society and made contact with O. M. Miller, who was developing a new technique for making charts by oblique photography from

the air. What had started out as a pleasure cruise soon developed, therefore, into a scientific expedition in the 97-foot schooner *Ramah*, with a crew of sixteen including a geologist and a botanist. Alex was owner and skipper of the ship, and did a splendid job of piloting her through uncharted, perilous waters into safe anchorages among the islands and inlets of the whole Labrador coast as far north as Cape Chidley. Two airplanes were attached to the expedition for aerial reconnaissance and photography. The ship ran aground twice without serious damage. The ship supplied a base for aerial operations and for a crew to establish fixed markers on convenient mountain tops, the positions of which were tied together by triangulation for accurate location of the aerial photographs. In spite of innumerable handicaps the expedition obtained the necessary photographs and ground markers to permit the completion of the desired maps. The survey was further extended by another expedition in 1932 in which Alex was unable to participate, but in 1935 he went north again by plane, this time as relief pilot and photographer, with only a pilot-mechanic as companion. After eleven days they completed their task of photographing the region near Cape Chidley, on the northern tip of Labrador, and returned to Boston. Just south of Cape Chidley is McLelan Strait, the western entrance to which was named "Forbes Sound" after Alex, by the Geographical Board of Canada.

Dr. Forbes served in the U.S. Navy during both world wars. He began in World War I as a Lieutenant (j.g.), commanding for two months a small patrol boat in the approaches to Newport Harbor. He was then detached for research on radio receivers for airplanes, and was appointed Radio Officer on the scout cruiser *Salem*. Eventually (in February 1918) he was assigned to radio compass duty and sent aboard a destroyer to take charge of installing and getting results with this new equip-

ment in the destroyers and cruisers operating in European waters. This was an invaluable and welcome experience which not only familiarized him with techniques of electronic amplification for later use in physiology but also stimulated his imagination. The result of the latter was a book entitled *Radio Gunner*, which he wrote and which was published anonymously by Houghton Mifflin Company in 1924. This was a fictitious account of an imaginary future world war in which a young physicist has a job in the Navy similar to the one enjoyed by Alex. In the story he also manages to maintain private communication with a Harvard classmate, who is Secretary of the Navy, and between them they have exciting adventures, and contribute substantially to the winning of the war by the skillful use of new electronic direction-finding equipment installed on the battleships. The war which Forbes described in the book turned out to be remarkably similar to the World War II which actually happened. The story had also some further basis in fact, as is illustrated by a story about Alex, kindly contributed by a Navy shipmate of those World War I years, Mr. Laurence M. Lombard.

According to this story, Alexander Forbes, then a Senior Lieutenant in the U.S. Naval Reserve, had been assigned to Queenstown, Ireland, to install the newfangled radio direction-finders on U.S. destroyers. "During the weeks that followed, Alex, with his friendliness, informality, and brilliance (and perhaps his nonmilitary appearance, even when in uniform), became an amusing and respected character among the destroyer officers. They couldn't quite make him out. Unimpressed by, but respectful of, rank, he was equally as quick to recognize ability in the seaman or the electrician 3d class with whom he might be working as in a high-ranking officer."

The scene of the episode in question was in the harbor of Queenstown. It was to be a gala day because the Assistant

Secretary of the U.S. Navy was coming to inspect the U.S. forces. The big ceremony was to take place on the U.S.S. *Melville*, which was a floating machine shop of some 20,000 tons, and officers of all the ships of the fleet were ordered to attend.

"On this particular day, apparently oblivious of the ceremony about to take place, Alex was working with his instruments down in the depths of the *Melville*. On deck the crews in their dress uniforms were lined up at attention. The inspection party headed by Franklin Delano Roosevelt, the Assistant Secretary of the Navy, was just being piped on board. The bugle was sounding, all hands were at salute, and all eyes were on the gangway where F.D.R. was stepping on board. All eyes, that is, except those of a few sailors across the deck where, emerging from a hatchway, appeared the figure of Alex Forbes, hatless, disheveled, and in loose-fitting dungarees, apparently coming up for air. Surprised at the sound of the ship's band, the gold braid of the officers, and the lines of sailors in dress uniforms, Alex looked around in astonishment. At just this moment the eyes of F.D.R. and his Harvard classmate met. 'Why, hello, Alex.'—'Hello, Franklin.' The tension was broken, all hands could smile as the Assistant Secretary of the U.S. Navy shook hands and chatted with Lieutenant Forbes. And, as the Lieutenant in dungarees was introduced to the First Lord of the British Admiralty, Alex's stock went up another notch in the eyes of the Queenstown flotilla."

At the close of that war, Alex returned to his post in the Department of Physiology at Harvard, interrupted only by his summer survey of the coast of Labrador. When World War II broke out he joined the Navy again, at the age of fifty-eight, this time as Lieutenant Commander, and was first assigned to the Pensacola Navy Laboratory to investigate the use of electroencephalography (brain waves) as a test for airplane pilots. Shortly thereafter, however, because of his familiarity with Lab-

radar he was assigned as "technical adviser" to a reconnaissance expedition in that region commanded by Captain Elliott Roosevelt. The purpose was to select locations for airfields so that fighter planes could fly under their own power to England via Iceland, to avoid the risks of submarine attack. This expedition lasted about three months and three different sites were recommended.

On his return to Washington he brooded over the navigational difficulties of ships trying to reach the sites selected for the construction of airports, and wrote a memorandum to the War Department recommending an expedition to chart the necessary channels. He had special concern for the site chosen on the Koksoak River flowing north into Ungava Bay. The result of this memorandum was that he was assigned to the Army Corps of Engineers for survey duty on the Koksoak River, but the orders were changed after his departure, and he ended up with four months in the approaches to the Frobisher Bay station. He spent the next few months exploring the bay, charting the channels, and piloting big ships to their destination through those perilous waters.

Back again in Washington, he spent the winter helping to put his data to use in making maps, and he even had his wife helping in drawing the charts. By June 1943, however, he was again flying north to the Koksoak River station, as officer in charge of a hydrographic survey. This was a most unusual assignment for a medical officer, but one which he was well fitted to undertake. Here the problem was to set up a network of tripods on the shore which could be located accurately by triangulation, and to make soundings of the channel. Part of the time he was sleeping in uncomfortable little tents on the shore, with swarms of mosquitoes, and one night he spent in an army MT boat, stuck on the rocks until the tide came in. In the boat there was no place to sleep, but he reports that

"I stuffed myself part way down the hatch and toward sunrise managed to doze a bit." By the end of summer he was back in Frobisher Bay for another hydrographic survey, returning to Washington in October.

The account of his rugged seafaring experiences on this trip was published in his book *Quest for a Northern Air Route* (Harvard University Press, 1953). In that book he writes, "Few indeed are the fields of human endeavor in which real manhood is displayed to better advantage than in good seamanship." The statement was intended to express his admiration for one of the skippers with whom he worked, but, according to this criterion, Dr. Forbes himself easily met the test of "real manhood" in every way. Airports were actually built as a result of this effort, but it was a little frustrating for the participants that in the end they were never actually used very much for the purpose for which they were built.

Back in Washington, Alex continued working in the Hydrographic Office, collecting better instruments for making maps from the mass of data which he had collected in the north, and sometimes preparing maps for emergency military landings in the Pacific, or preparing a "ponderous" report on the dissipation of smoke screens. This report, like others from Labrador, was never used during the war, and Forbes wrote in his book, "It was my lot, I believe, to be involved in more of these fortunate futilities than any other man in the Navy."

Then came the end of the war and the atom bomb test at Bikini. Alex was sent out on the Operation Crossroads mission to apply his familiar techniques to mapping and measuring the waves generated by the explosion. In a book by David Bradley, describing these tests (*No Place to Hide*, Little, Brown and Co., 1948), a story is told of the day when Alex set out to inspect the various cameras set up to record the explosion. A "big, burly, somewhat overfed Marine photog-

rapher" who was assigned to accompany "the white-haired gent" came back at the end of the day prostrated with exhaustion, for as soon as he reached a wharf Alex "leaped ashore and headed straight cross-country, taking the dense underbrush at an easy lope. Up the first tower he scrambled as though taking in the topsails for a squall. By the time the puffing Marine had struggled to the top, Dr. Forbes had finished inspecting the cameras and their radio-control mechanisms, and was on his way down." And so it went for the rest of the day. Similarly, in the log of the schooner captain in Labrador, Alex was referred to as "O.G.," which stood for "old gent." With his white hair and hearing aid he looked older than he acted.

His years of duty in the Navy terminated in Washington in 1946. He was promoted to Commander in 1943 and to Captain in 1945. He returned to Boston in time to be retired as Professor Emeritus in 1948. In spite of all the excitement of his years in military service he found no difficulty in resuming his work in physiology with the same zest as before, and completed the publication of eighteen more scientific papers with eighteen different young collaborators, mostly on the electrical changes in the retinas of frogs, turtles, and lizards when exposed to lights of varying wavelengths. He also had an opportunity at this time to record for the first time a slow change in potential of one millivolt in the human mammary gland when stimulated either by the act of nursing or by the injection of pituitrin.

Although Dr. Forbes was never very active in civic and political affairs, he often was concerned enough regarding political issues to write hot letters to newspapers, to congressmen, and to his old friend and classmate, Franklin Delano Roosevelt, during the latter's Presidency. (It is reported that he never voted for F.D.R.) He did, however, become very much

interested in the George Junior Republic in Freeville, New York, and spent many hours and much money in helping that institution to train boys and girls for responsible citizenship. He continued his interest in the institution until his death, and was President of the Board of Directors for thirty-six years. In 1964 he was presented with a testimonial of admiration and gratitude from the institution, dedicated to "a great, good, and lovable man as he begins his sixtieth year of deep commitment to the unchanging purposes of this organization."

Dr. Forbes's own appraisal of the George Junior Republic can best be described in his own words as follows: "This educational project, founded by William R. George in 1895, has had a most profound influence on educational methods in training adolescent boys and girls by placing the responsibilities of self-government and self-support squarely on their shoulders. Thus it was the chief pioneer in pointing the way toward such self-government as has been adopted in schools throughout the land. The success of this method as a therapeutic measure in the case of certain maladjusted young people has awakened the interest of psychiatrists as well as educators. Most noteworthy of all has been its contribution to responsible citizenship." Alexander Forbes was not blind to social problems and civic responsibilities.

Alex was always the life of the party in any gathering anywhere, and seemed to be as much at ease in the White House as in an Eskimo hut in Labrador. His rich and varied experiences gave him a wealth of material for good stories which he told with great good humor. Mixing as he did good solid science with a love of somewhat risky and ambitious undertakings on the high seas, on the mountain peaks, or high above the clouds in an airplane, he represented a type which is seldom encountered in the world today. He tells of picking up a

textbook of physiology in an isolated outpost in Labrador in which he found an account of some of his own earlier work on reflex conduction. He writes that he found great difficulty under those circumstances in recognizing himself as the scholar who could have carried out the investigations which were recorded. The scientific and the adventurous were, however, the two sides of his character. With less opportunity for such colorful but useful adventures he would have contributed even more to science than he did, but his total contribution to his friends and to the country would have been far less. We loved him for what he was, and will never forget his enduring enthusiasm which seemed so easily to overcome all the obstacles that lay before him and endeared him to all who knew him.

ACKNOWLEDGMENTS

IN PREPARING this biography I have borrowed freely from less extensive memoirs previously published by Dr. H. Davis (*J. Neurophysiol.*, 28:986, 1965, and *Electroencephalog. Clin. Neurophysiol.*, 1:139, 1949), by Dr. John R. Brooks (*Harvard Medical Alumni Bull.*, May 1959), by Dr. Richard H. Miller (*Harvard Medical Alumni Bull.*, 1949), by Lord Adrian (*Electroencephalog. Clin. Neurophysiol.*, 19:109, 1965), and by Professor John R. Pappenheimer and the members of his Harvard Medical School Committee who prepared the official memoir for the University (*Harvard University Gazette*, 61:32-33, 1965). I have had access also to the autobiographical statements which Dr. Forbes himself wrote for his Harvard Class of 1904 reports and for the National Academy of Sciences (unpublished). For help in the collection of pertinent material, may I thank also Mrs. Joseph R. Frothingham, daughter of Dr. Forbes; my brother, Roger C. Fenn, of Concord, Massachusetts, and my nephew, Donald Duncan, of Milton Academy; as well as other friends mentioned in the text: Laurence M. Lombard, John R. Perkins, Jr., and especially Hallowell Davis, who has been particularly helpful in amplifying my account of the scientific work of Dr. Forbes.

BIBLIOGRAPHY

KEY TO ABBREVIATIONS

- Am. J. Physiol. = American Journal of Physiology
Electroencephalog. Clin. Neurophysiol. = Electroencephalography and
Clinical Neurophysiology
Fed. Proc. = Federation Proceedings
J. Appl. Physiol. = Journal of Applied Physiology
J. Med. Res. = Journal of Medical Research
J. Neurophysiol. = Journal of Neurophysiology
J. Opt. Soc. Am. and Rev. Sci. Instr. = Journal of the Optical Society of
America and Review of Scientific Instruments
J. Physiol. = Journal of Physiology
Photogrammet. Eng. = Photogrammetric Engineering
Physiol. Rev. = Physiological Reviews
Proc. Nat. Acad. Sci. = Proceedings of the National Academy of Sciences
Rev. Sci. Instr. = Review of Scientific Instruments

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W. P. Kelly

WALTER PEARSON KELLEY

February 19, 1878–May 19, 1965

BY HOMER D. CHAPMAN

THE DEATH OF Walter Pearson Kelley terminated more than sixty years of productive research and scholarship in the field of soil science. During this period (1904 to 1965), an extensive body of knowledge concerning the genesis, nature, properties, variability, and management of soils came into being. Kelley was among the distinguished contributors to this science, and his name will be forever linked to alkali soils, ion exchange, and clay mineralogy. Two widely known books and nearly one hundred research papers were published by him and his colleagues.

He was born on a tobacco farm near Franklin, Kentucky, on February 19, 1878, and died in Berkeley, California, on May 19, 1965. He was the son of John William Kelley and Mary Eliza (Mayes) Kelley. His father was of Scotch-Irish ancestry, a descendant of immigrants to North Carolina in early colonial times, and his mother was of English ancestry, a descendant from immigrants to New England in early colonial times.

Kelley was educated in the elementary public schools of Simpson County, Kentucky, and a private high school. He graduated from the University of Kentucky in 1904 with a B.S. degree in chemistry. His intention was to go into medi-

cine, but the necessity of finding work led him to take a job as Assistant Chemist at the Purdue Agricultural Experiment Station. He spent three years there, engaged both in study and in the routine analysis of agricultural materials. He got his M.S. degree at Purdue in 1907. In 1908 he was appointed by W. H. Evans, then Chief of Insular Affairs, Office of Experiment Stations, U.S. Department of Agriculture, to direct research in agricultural chemistry and soils at the Hawaii Agricultural Experiment Station. This station subsequently became a part of the University of Hawaii. It was here that the interesting problems and challenges of agriculture influenced him to devote his life to serious research in this field.

In 1911 Kelley enrolled as a graduate student in soils at the University of California, Berkeley, where he got his Ph.D. degree in 1912. His major professor was E. W. Hilgard. Kelley returned to Hawaii to continue his research, but soon thereafter he was invited by Dr. H. J. Webber, Director of the newly established Citrus Experiment Station and Graduate School of Tropical Agriculture at Riverside, California, to become Professor of Agricultural Chemistry and head of that department. He remained in this position until 1939, and much of the research for which he became famous was carried out during his twenty-four years in Riverside.

With the administrative reorganization of the Department of Soil Technology at Berkeley, Kelley became chairman of the Division of Soils on that campus in 1940 and continued his research until retirement in 1948 as Emeritus Professor of Soil Chemistry. Though his active involvement in research slackened somewhat following retirement, his keen and relentless interest in soils (and particularly clay mineralogy, cation-exchange phenomena, and alkali soils) continued unabated. It was following retirement that he wrote two books—one entitled *Cation Exchange in Soils*, and the other, *Alkali Soils*:

Their Formation, Properties, and Reclamation—both American Chemical Society monographs.

My personal association with Kelley began in 1927 when I joined his staff at Riverside, and for the following twelve years it was my pleasure to work in close association with him, though my assignment was in soil fertility and citrus nutrition. But even after he transferred to Berkeley we kept in close touch by letter and frequent visits right up to the time of his death. Thus it was my privilege to have known him well for a period of nearly forty years. Though the years gradually slowed him down physically, his mind remained razor-sharp to the very end. A close friend in Berkeley, Mr. J. E. Tippet, talked with him just a few hours before he passed away; he said that Kelley was mentally alert, though in some physical pain and discomfort.

Kelley had planned to attend the June 1965 meetings in Riverside of the Western Society of Soil Science, of which he was a charter member, and I had several letters from him just a few weeks prior to his death about his forthcoming visit. Plans were made to take special note of his attendance and to call on him for a few remarks. Perhaps anticipating some of the comments he might make, he wrote to me on April 14, 1965, and I quote in part from this letter.

“Thinking back over the period of my activity, this period was certainly one of notable advancement in the field of soils. In fact, it is somewhat difficult to realize what these advances have been. In my opinion, nothing equal to this period has ever taken place in the whole history of soil research. These advances pertain both to the soil itself and to plant nutrition. Regarding the latter, practically the whole of our knowledge concerning the role of minor elements has come to light. In regard to the soil, the most comprehensive understanding of the actual constituents of the soil has been developed.

"It was only near the close of the second decade of the 20th century that a realization began to dawn as to the important role of cation exchange. Out of this has grown definite knowledge as to the most active and important constituents of the soil. Until this study had developed for about ten years, it was almost universally held that the so-called clay of soils is amorphous. So long as this view prevailed, the vast majority of the work was necessarily empirical, for the reactions were taking place with unknown substances. Now we know what these substances really are and are in a position to really understand what reactions take place; why different soils react differently; why one and the same treatment produces variable results on different soils; and the agricultural significance of the results is susceptible of rational understanding. Perhaps the most important of all these advances applies directly to irrigated soils; cation exchange has different effects, agriculturally speaking, with different soils, owing to the fact that the reacting substances are different.

"In a certain sense, it might be thought that the soil, in consequence of these researches, has become increasingly complex, for the interpretation of experimental data was formerly not encumbered with too much knowledge; sweeping conclusions could be and were, as a matter of fact, drawn on a purely empirical basis. It is always easier to explain if too many facts are not in the way. Much more could be said about this period. Suffice it to say, I am happy to have been rather closely involved in many of these advances. I wonder if corresponding advances will accrue in the next half century. Please excuse me for this digression.

"As I wrote previously, there is a possibility of my being able to attend the meetings in Riverside next June. It has occurred to me that at the meeting of the Western Society of Soil Science would be a suitable occasion to inform the younger

members about the origin and early held views of the founders of the Society. It seems probable that I am the only surviving one of the first meeting in Salt Lake City, called, incidentally, at my request for another purpose."

While Kelley is best known for his research on alkali soils, base exchange, and clay minerals, his inquisitive mind, capacity for organized research, deductive reasoning, and keen powers of observation are readily apparent in his very first investigation in Hawaii. This research had to do with a chemical study of soils in which pineapples became chlorotic, made poor growth, and were largely unfruitful. He found that these soils (black) were extraordinarily high in total manganese, with values ranging from 3.91 to 9.74 per cent Mn_3O_4 , as compared with 0.15 to 0.91 per cent in red soils, where pineapples grew well. Although previous studies had shown some Hawaiian soils to be high in manganese, Kelley was the first to establish a relation between the aforementioned pineapple disorder and a high manganese content of soils. He also suggested the possibility that the yellowing effect of the manganese might in some manner be related to an effect on iron metabolism. Subsequent findings by others proved this to be the case.

Kelley's was really the first thorough description of the effects of excess manganese on the growth, appearance, and fruit characteristics of the pineapple. He likewise noted the appearance of corn, rice, oats, wheat, beans, peanuts, cotton, sugar cane, and other plants in these high manganese soils. Stunted growth, chlorosis of foliage, burn on older leaves (followed by abscission), and purplish or reddish coloration of leaf sheaths and stems were some of the effects described. He noted that the condition on pineapples and other plants was worse in winter, tending to improve or clear up in summer. He observed great differences in plant sensitivity to excess manganese, sugar cane being much less affected than pine-

apples. Other sensitive plants were rice, cowpeas, peanuts, kidney beans, pigeon peas, corn broom, corn, and sorghum.

He made ash analyses of many of these plants and found in some a marked tendency to accumulate manganese in the leaves and stems and, in others, very little accumulation. He discovered that the composition of other elements was commonly greatly altered when the plants were grown in manganiferous soils, calcium tending to increase while magnesium and phosphorus decreased.

Kelley's passionate interest in soils and soil-forming processes and his inquisitive nature were revealed by his studies to determine the mode of origin of the manganiferous black soil areas of irregular shape and size occurring on the alluvial plateau of Oahu. After a thorough study of the location and depth of these deposits, the chemical composition of the original lava and of the soil resulting therefrom, plus the geology of the islands, he concluded that the manganese enrichment resulted from the dissolving action of water, the transport of manganese in solution to lower levels, and the subsequent oxidation and deposition around various nuclei of soil or rock particles. The manganese concretions found in these soils strongly suggested that in the geological past the lower alluvial plateaus had had a history of submergence; he noted in this connection the findings of manganese concretions scattered promiscuously near the floor of the ocean at various places between Hawaii and Japan and to the south of Hawaii. These deposits were discovered and reported in connection with the voyage of the H.M.S. *Challenger* in 1873-1876.

In rereading Kelley's early publications on manganese, one notes several traits which marked all his writing and research activity. First, his ability to write and express himself clearly and logically; second, his capacity for thoroughness. This latter trait is revealed in his extensive and knowledgeable review of

existing literature (both old and current), plus the prodigious amount of analytical work carried out (he was aided in this, of course, by assistants) in connection with his inquiries. Third, his ability to single out and spend time on the significant, as against the pursuit of the less important or insignificant.

In his six years in Hawaii, some twenty major pieces of research were completed. In addition to his manganese studies, extensive chemical analyses of Hawaiian soils and of rice and pineapple plants were made; field fertilizer experiments with rice were conducted; and, growing out of the latter, extensive studies were carried out on the humus of Hawaiian soils and on various nitrogen and humus problems.

Some of his first nitrogen work was with rice, in which he showed that ammonium sulfate gave better growth and yields than equivalent amounts of sodium nitrate. The poor results with nitrate fertilizer, especially with young rice, were due in part to leaching losses from the soil, denitrification, and formation of nitrite, which if it reached levels of 5 ppm or over made the rice yellow. He also hypothesized that in young rice there may be a lack of nitrate-reducing enzymes.

Extensive investigations of nitrification and ammonification in Hawaiian soils were made, and he noted the importance of aeration and lime in the development of nitrate in soils. Studies were made of the rates of ammonification of various organic nitrogen compounds. He discovered that casein underwent much more rapid ammonification than dried blood, soybean cake meal, cottonseed meal, and linseed meal, and he concluded that basic diamino acid nitrogen compounds were converted into ammonia more rapidly than the nitrogen of other compounds.

In further studies of nitrification, Kelley found that, while calcium carbonate additions increased ammonification and nitrification of dried blood added to soils, magnesium car-

bonate decreased ammonification, inhibited nitrification, and brought about actual ammonia losses from soils. Calcium carbonate did not prevent or decrease the action of magnesium carbonate. Additional research with various Hawaiian soils revealed that magnesium carbonate stimulated ammonification in some and was toxic in others. Natural dolomite had effects similar to calcareous limestone. In later California work on nitrification, he noted that under alkaline conditions large amounts of nitrite sometimes formed.

Kelley's interest in nitrogen transformations, gains, and losses in soils persisted, and he was responsible for the installation of twenty-four large lysimeters at Riverside, in which he planned to conduct further nitrogen research. However, he became so involved in cation exchange and alkali problems that it remained for the author of this memoir to plan and execute the lysimeter nitrogen research which Kelley had hoped to get started.

In recognition of his interest and research on nitrogen, Kelley made a trip to Europe in 1930 under the joint auspices of the American Society of Agronomy and the Chilean Nitrate Educational Bureau to study nitrogen fertilization problems. His report on these observations was published in 1933.

In much of his Hawaiian work, Kelley was materially aided by W. T. McGeorge and Alice Thompson. McGeorge has written a very interesting account of his early experiences with Kelley in *Soil Science*, 97:76-79.

Shortly after his arrival in California in 1914, Kelley's energy and interests became channeled into various citrus problems. He was appointed chairman of a staff committee on field experiments and helped plan a 40-acre, long-term field fertilizer experiment with oranges. While the management of this experiment was turned over to others, his interest in it remained active during its entire course. The author was brought to

California by Kelley to grow up with this fertilizer experiment and to carry on collateral citrus nutrition studies.

The probable origin of Kelley's great and lasting interest in alkali soils, cation exchange, and clay mineralogy, in which one thing led in more or less logical sequence to another, was the increasing incidence of alkali injury to lemons and oranges and the prevalence of "mottle leaf," which appeared to be in some way related to the alkali problem. Hilgard and Loughridge in California had earlier noted the ill effects of saline irrigation waters on citrus, but their investigations were, in general, of a qualitative nature. Kelley and his colleagues, E. E. Thomas, S. M. Brown, and A. B. Cummins, proceeded systematically to investigate the problem. The first irrigation water survey was carried out by Kelley and Brown, and some thousand samples from widely scattered citrus areas were analyzed. He noted that wells and surface waters close to major watersheds were, in general, low in salt, but wells located at some distance from these watersheds often (but not always) showed notable amounts of salt. The soils of many citrus orchards irrigated with waters of varying quality were analyzed for their salt content, and tree condition was related to both the soil and irrigation water analyses. As a result of this work, it became possible roughly to classify waters as to suitability for citrus irrigation, and in the years that followed an enormous number of well waters were analyzed for farmers in many parts of the state and an extremely valuable service was rendered to agriculture generally. Kelley and his colleagues noted a high nitrate content of well waters in some regions—a finding which has both interested and puzzled investigators ever since. No final explanation of the source of nitrate in many of these waters remote from agricultural areas has as yet been uncovered, though a number of theories have been propounded.

Kelley added considerably to the descriptions of salt injury

to citrus, noting the yellowing, burning, and premature abscission of leaves from affected trees, and their greater susceptibility to frosts, heat, and winds. The salt content of composite soil samples taken from irrigated middles in orchards showing severe injury was around 1,000 ppm. He noted, as have many later investigators, the buildup of salts in furrow crests and in the unirrigated spaces under the trees. Salts move laterally into such zones by capillarity.

It was in connection with this work that Kelley observed that sodium was absorbed by the soil from some of the waters and calcium was released; further, that in these instances the physical condition of the soil was altered. He found, as had Hilgard and others much earlier, that gypsum applications would somewhat ameliorate this condition and help improve orchards. Kelley repeatedly stressed in his early studies the need to apply sufficient irrigation water to flush the salt residues out of the root zone, cautioning, however, that this might raise water tables in some areas and make a bad situation worse.

The aforementioned work with citrus led Kelley into a greatly expanded program of alkali research. He was encouraged and supported in this by the then Dean of the College of Agriculture and Director of the Experiment Station, Thomas F. Hunt. While recognizing that irrigation waters contributed significant amounts of salt, he emphasized and stated repeatedly that the thousands of acres of land which in California and throughout the world had become salt-impregnated were largely the result of the capillary rise of salt from high water tables, the latter a consequence of seepage from unlined canals and laterals plus overirrigation. He was quick to recognize the importance of installing an adequate system of drainage to carry away excess water and salt, both as a preventative of alkali formation and as the only means of reclaiming land made unproductive by salt impregnation. In all of this he was fully

aware of the previous distinguished investigations of Hilgard and others on the subject.

The impermeable soil conditions which existed when black alkali was present and the difficulties of getting water to pass through such soil even where gypsum had been applied led Kelley and his associates to make special studies of this problem.

During the same period, Kelley's attention had been drawn to a fertilizer plot at the old Rubidoux Station at Riverside, which had been receiving sodium nitrate at rates of about one thousand pounds per acre. The citrus trees in this plot had developed severe "mottle leaf," and the soil had become hard and took water poorly. The condition of the soil and trees in this plot, together with the aforementioned black alkali conditions, led Kelley and his associates to make extensive laboratory studies of the chemical effects of salts on soils.

His first research paper on this subject was published in the February 1921 issue of *Soil Science*. He and his assistant, A. B. Cummins, treated two different soils with equivalent concentrations (0.01 normal) of the chlorides (also sulfates) of sodium, potassium, calcium, magnesium, and ammonium, and noted that an essentially equivalent exchange of bases took place, with the result that a portion of the base of the added salt passed out of solution and a chemically equivalent amount of other bases was set free from the soil. In this process, there was no essential change in the concentration of the added anion. He noted that these reactions were reversible, that concentration played a part, and stated that apparently the reactions obeyed the principle of mass action. Further, the subsequent distilled water leachate from the sodium-chloride-treated soils was turbid and dark-colored, and the soil became progressively impermeable to water as the free salt was leached out. From this and collateral studies, he quickly deduced that the

essential difference between black and white alkali soils was that the former had absorbed considerable amounts of sodium, and it was this fact which accounted for their impermeability and black color.

These studies led Kelley into a search of the literature, and he discovered that Thomas Way of England was apparently the first (1850) to note and write about the exchange reaction in soils. Way's work was followed by that of several German workers—notably Eichhorn (1858), Henneberg and Stohmann (1859), Lemberg (1870-1877), Peters (1860), and Van Bemelen (1888). A review of much of this early work was covered by Sullivan in 1907.

Very shortly after this first paper on cation exchange by Kelley and Cummins, the extensive work by Gedroiz in Russia and Hissink in Holland came to Kelley's attention. English translations of the Gedroiz papers were made by S. A. Waksman and became available to Kelley and his co-workers through C. S. Scofield, of the U.S. Department of Agriculture. All of these investigations added fuel to Kelley's intense research activities in this field, and there followed during the next twenty years a succession of papers dealing with one or another aspect of cation exchange, the nature of the clay fractions in soils, and the varying physical and chemical properties of soils containing different kinds and proportions of exchangeable bases.

A great deal of this early work was motivated and closely tied in with his alkali reclamation research, particularly on soils containing sodium carbonate. Though he found it possible under laboratory conditions to quickly leach out chlorides, nitrates, and (to a lesser extent) sulfates, the problem of ridding soils of carbonate and bicarbonate led him and his colleagues into studies of the generation of sodium carbonate in soils. This problem and conflicting theories about it held his interest for many years.

Early work by Kelley and Cummins demonstrated that the hydrolysis of sodium clays in soil systems where soluble electrolytes had been removed was responsible for the formation of sodium hydroxide, which (reacting with CO_2 of the air) resulted in the formation of sodium carbonate. He was, of course, well aware of the sodium sulfate-calcium carbonate reaction.

In connection with studies of cation exchange, considerable effort went into investigations of methods for determining the exchangeable bases and the cation-exchange capacity of soils. Throughout his papers there is more or less discussion of the exchange phenomena, whether the replaceable bases were held by some sort of physical adsorption or in true chemical combination. Kelley from the first (and long before the existence of more knowledge of the crystalline nature of these compounds and the residual electrical charges on the edges and surfaces of these crystals which account for the adsorption of positively charged ions) held to the view that the bases were tied by true chemical binding forces and that, in the main, behavior was in accord with that obeyed by all chemicals. In his studies of methods, he was quick to appreciate the complications posed by the solubility of other materials in soils, silicates, lime, dolomite, and oxides, and concluded that highly precise determinations of total exchangeable bases and exchange capacity were difficult, if not impossible.

Kelley recognized the special suitability of ammonium chloride as a replacing agent, and for many years normal solutions of this chemical were used by workers all over the world, to replace both the bases of soils and (by determining the amount of ammonium ion retained by the soils) their so-called exchange capacity. Later, others proposed the use of ammonium acetate, and this latter method is still widely used.

In acid soils, Kelley noted that neutral normal ammonium chloride brought aluminum, iron, and manganese into solution

and these he then regarded as exchangeable bases. He was, of course, fully aware at this time of the concept that the hydrogen ion could also replace the various bases and in acid soils was responsible, at least in part, for soil acidity. The question of whether the trivalent bases brought into solution by neutral salt extraction had preexisted as replaceable bases or were dissolved from the soil by the replacement of hydrogen and the formation of strong acids was discussed by Kelley. His experiments with a few soils at the time led him to believe that, in some soils at least, the trivalent bases preexisted in replaceable form. It is interesting that much recent work has confirmed this view, especially in regard to aluminum.

There was much speculation during the early 1920s, and earlier, too, about the actual nature of the finely divided alumino-silicates responsible for base exchange in soils. Speculating on the reasons why a part of the replaceable calcium, and possibly other bases as well, are more easily removed than others, Kelley foreshadowed later findings by hypothesizing that differences in replacement rate might be due to "the occurrence of molecular aggregates of the replaceable compounds, submicroscopic crystals in fact, some of whose chemically combined bases occur on the interior of the particle and can be replaced only as a result of diffusion." This statement was published in a 1924 paper. In ensuing years, Kelley became interested in bentonites and their high base-exchange capacity. The findings of Ross and Shannon, that some bentonites contain a finely divided micaceous crystalline mineral (montmorillonite), led Kelley to grind bentonites finely in a ball mill, with the result that the base-exchange capacity was greatly increased; in some bentonites, much of the originally nonreplaceable magnesium became replaceable. Also in some bentonites, considerable replaceable potassium appeared as a result of grinding. These results led Kelley to the separation

and grinding of soil colloids. Results remarkably similar to those with bentonites were obtained. Not only was there a great increase in exchange capacity, but both replaceable magnesium and potassium (and in some cases hydrogen ion) appeared subsequent to the grinding. These results led Kelley to the conclusion that the base-exchange constituents of both soils and colloidal bentonites are crystalline and that the replaceable bases are contained within the interior of the crystals, as well as on the exterior. To secure further evidence, both soil colloids and bentonites were subjected to X-ray diffraction examination, and the case for the crystallinity and similarity of both was confirmed. Chemical analyses of a wide range of soil colloids had led Holmes, of the U.S. Department of Agriculture, to suspect that the basic materials of these colloids might be crystalline, and in a paper submitted to *Soil Science* on November 29, 1929 (just four and one-half months before the paper by Kelley, Dore, and Brown was submitted to the same journal), Hendricks and Fry presented extensive X-ray diffraction data on colloids from many soils indicating that they contained crystalline substances of a nature similar to those of the montmorillonite and beidellite clays.

The results of these investigations laid to rest many previously held ideas, such as, for example, that the inorganic base-exchange material of soils was amorphous; that all soils contained just one principal component; that the replaceable bases were held by some vaguely defined adsorption force; that the material was a zeolite or zeolite-like substance. In place of these concepts, it became clear that the inorganic colloidal components of soils having base-exchange properties were crystalline in nature, were definite chemical compounds, and were identical with a number of the well-known clay minerals such as montmorillonite, kaolinite, and others. Also, that the cations, principally magnesium, potassium, or a combina-

tion of these, occurring primarily in the interior of these crystals would become replaceable if the crystals were broken up by grinding.

This work led to an expanding series of investigations, not only with X-ray equipment, but with the electron microscope, with the result that the nature and identity of the inorganic base-exchange components of soils are now much better understood.

Kelley and his associates continued their work on various questions, such as the kinds of bonds with which exchangeable bases are held, the reasons why some minerals (even when finely ground) possess little or no base-binding capacity as compared with those that have a high capacity. All of this led him into a thorough study of crystal structure and the role of aluminum, magnesium, and hydroxyl ions in relation to the existence of negative electrical forces on the platy surfaces and edges of crystals which attract and bind cations with varying degrees of force or tenacity. Also, he was much interested in the mode of origin and the properties of the clay minerals in soils, and his active research in this matter continued until his retirement in 1948. Moreover, his special interest in this subject continued actively even after retirement, when for a period of years he was retained as a special consultant by the Gulf Research and Development Company of Pittsburgh, Pennsylvania. For a period of some six years, he spent part of each year at the research headquarters of this company in an advisory and consulting capacity.

In his work at Riverside from 1925 on, Kelley was ably assisted in the physical chemistry of the exchange phenomena by Dr. A. P. Vanselow and in mineralogical aspects by A. O. Woodford and W. H. Dore. Kelley's initial interest in salinity and alkali problems, as already indicated, was related to citrus, but in 1919 he was given the responsibility of seeing what

could be done about reclaiming lands which had become salt-impregnated on the Kearney vineyard property in Fresno County, California. Previous attempts in 1914-1915 to reclaim the land through installations of tile drains and by flooding were only partially successful. It was this and his previous work on citrus which led Kelley and his associates, S. M. Brown, E. E. Thomas, and A. B. Cummins, to initiate his basic work on soils and, at about the same time, to set up a carefully laid out alkali reclamation experiment in the Kearney vineyard. The affected soil in this area was black alkali, and it was Kelley's extensive studies of the effects of salts on soils which led to the discovery that the difficulties of reclaiming this type of soil were due to the fact that the sodium salts had reacted with the clay components to form sodium clays. As already discussed, the generation of sodium carbonate and the tendency of such soils to become impermeable upon leaching away the accompanying soluble salts, together with a great amount of other information (now all common knowledge, but previously unknown), were revealed by his research on this subject. Kelley and his co-workers arrived at this information independently, though (as already stated) he subsequently learned of Gedroiz's series of papers on the subject, as well as the work of a de Dominicis in Italy in 1918.

While the value of gypsum as a means of ameliorating black alkali soils had been known for a long time, it was not appreciated until Kelley's research indicating that, in addition to its direct action in converting Na_2CO_3 to Na_2SO_4 and CaCO_3 , a part of its beneficial influence was in changing sodium into calcium clays. The successful outcome of the reclamation work on the Kearney vineyard soil was due basically to the aforementioned discovery, as well as to the recognition that the existing tile drainage system was inadequate and the subsequent installation of a 70-foot well in the area. This well

lowered the water table sufficiently to permit the salts to be leached below the root zone of the crops being grown.

As with gypsum, the research of others had indicated that sulfur, sulfuric acid, iron, and aluminum sulfates might be useful in counteracting the sodium carbonate of black alkali soils, but a complete understanding of the effects of these materials in soils and their relative usefulness in alkali reclamation did not emerge until the work on the Kearney plots was carried through to completion, accompanied as it was by many detailed laboratory investigations. Kelley's research on alkali soils continued for some twenty years following his initial work, and his interest in various facets of the subject, especially the mechanisms by which sodium carbonate is generated in soils and the whole broad subject of the permanence of irrigated agriculture, continued.

While this brief account of Kelley's interests and contributions covers his most important work, mention should be made of some other research activities. He always had an active interest in the disorder known as citrus "mottle leaf," and suspecting it to be nutritional in origin, he and his associate, A. B. Cummins, made extensive analyses of normal and mottled citrus leaves. While the work revealed important compositional differences between mottled and green leaves, he and Cummins did not analyze for zinc (the lack of which eventually proved to be the cause of mottle leaf) and thus narrowly missed uncovering the reason for this disorder, which for so many years had puzzled investigators.

Of special interest were his findings on boron in relation to some citrus and walnut disorders. Both the essentiality and toxicity of boron had been well known to plant physiologists for a long time, but there had been no connection of these to citrus troubles until the latter part of 1925, when Kelley's attention was drawn to a mottling, necrosis, and excessive leaf

shedding of orange trees in the vicinity of some citrus packing houses. It was quickly found by soil and leaf analyses that the trouble was due to excess boron. Boron compounds are commonly used as fungicides in the packing house wash waters, and in some cases the discarded wash waters found their way into irrigation supplies. This finding led to the discovery that certain California irrigation waters naturally contain enough boron to produce toxicity symptoms on citrus identical to those seen near the packing houses. On the basis of these findings, Congress made a special appropriation to study the natural occurrence of boron in irrigation waters and soils of the western regions, and also the tolerance of a wide range of plants for this element. This work was initiated in Kelley's laboratory by F. M. Eaton, under the general direction of C. S. Scofield, both of the U.S. Department of Agriculture. Kelley and his colleague, S. M. Brown, were the first to establish the leaf levels of boron in normal and injured walnut leaves, the boron content of soil likely to produce injury in citrus and walnuts, and the levels of boron in irrigation waters which will produce injury. This research provided the basic data for the identification of excess boron troubles in many parts of the world.

This brief review of Kelley's research activities reveals that in all of his work motivation arose almost always from practical agricultural problems. However, he was never content with a superficial or partial solution. He always wanted to probe into the basic reason for things, and his attention was directed primarily to the soil and its chemistry. He did not attempt to pursue all aspects of soil and plant relationships, leaving the physiological side of plants, in general, to others.

A great deal of his success can be attributed to selection of and emphasis on aspects likely to yield results and avoidance of blind alleys. Thus, a great deal of insight, intuition, and sound judgment went into all of his research.

Though he pursued his special interests with remarkable tenacity and singleness of purpose, he kept thoroughly abreast of research in all other aspects of soils and read widely in the field. This is shown by his broad knowledge of soils, their complexity, and their variability.

Kelley was never content to publish a piece of research or embark on any new line of work without thoroughly reviewing the literature. In later years, he remarked to me repeatedly about the tendency of many younger researchers to do a superficial job of literature searching. His writing and research revealed true scholarship in the finest sense. He read not only widely but meticulously. He would puzzle over certain findings of others for days and was quick to single out the flaws or omissions in the research of others, as well as his own. Another outstanding trait, as already mentioned, was his clear and logical thinking. This is apparent in all his writings and was evident to the many with whom he conversed in his long life.

One of his close and long-time friends, J. E. Tippet, of Berkeley, commenting on some of Kelley's traits, wrote to me as follows:

"Over the years I had hundreds of conversations with Kelley concerning many different subjects. By and large, I found him to be just about the most objective man I ever knew. I never knew him to express a judgment based on his personal advantage or disadvantage, or based on a personal bias. He was always concerned with ideas, with concepts, and with things, rather than with the personalities of people. Even though he was a strong advocate of basic or so-called fundamental research, he felt that such research should not be limited to a scientific laboratory but should be related to the actual conditions of life and living. He genuinely believed that basic research could, in part, be conducted in the field where the particular problem existed, and therefore related very directly to the

problem. He was a well-read man in many fields other than soil science. His apartment home was literally full to overflowing with good books. Not only were the bookshelves filled, but books were stacked on tables, on chairs, and on the floor. Not only was he a good citizen, but he kept himself quite well informed in many fields, and he never expressed himself without a background of reliable knowledge."

Kelley loved conversation as well as reading and always searched for just the right word to express his meaning clearly. As indicated, his reading and conversation were by no means confined to his research. He was interested in everything around him, especially the main currents of political and social thought, the direction of national and international life, and the implication of social legislation. He was well informed on the affairs of the day and was never content with a superficial understanding of things, but strove constantly to seek for the undercurrents or motives behind the events of the day.

He was also frank and outspoken, not only as regards his research and that of others, but on practically any and every subject to which he turned his attention. Kelley was a lively and stimulating conversationalist, and while he loved to discourse at length himself, he was always willing to listen to anyone who had something significant to say or contribute. Likewise, he was always full of questions—the fount of good conversation.

Increasingly in his later years, Kelley loved to settle down in a chair in a laboratory or office, put his feet up on a desk or chair, light a cigar, and launch into some discussion about a phase of his or the other fellow's research. This would commonly lead to other items of interest, both related and unrelated. Over the nearly forty years of our acquaintance, I never saw him smoke either a pipe or a cigarette; cigars were his love.

Though Kelley played golf and badminton and engaged in lawn bowling at various periods in his life, not one of these activities ever became dominant or occupied much of his time. He was never an avid sports fan, though he followed sports (especially college football) with considerable interest. He played a good game of bridge, but again this was secondary; he always preferred reading when alone or conversation when with associates or a group.

He had a lively sense of humor and loved (and could tell) a good story. When especially entertained, he would laugh heartily, long, and with gusto—a likable and infectious trait. He was a genial and interesting companion on trips; the writer spent many pleasurable days in his company on visits to the Imperial Valley and other places of interest in California.

Kelley married Sue Katherine Eubank, of Oakland, California, on August 6, 1913. They had no children. Mrs. Kelley was a charming and gracious person and a sparkling, vivacious hostess. She took up oil painting shortly after coming to Riverside, and, accompanied by her husband, made many trips into the high Sierras, where she sketched and painted while Kelley relaxed, hiked, and read. They developed the habit of reading aloud to one another in later years, and when Mrs. Kelley's eyesight began to fail, he continued to read aloud to her. He was devoted and most solicitous of her and missed her greatly when she predeceased him by several years.

Kelley's achievements and research career won him widespread recognition and many honors. He was a member of the American Chemical Society, the American Association for the Advancement of Science, the American Society of Agronomy, the Soil Science Society of America, the Western Society of Soil Science, the International Society of Soil Science, the Western Society of Naturalists, the Geophysical Union, the American Mineralogical Society, and the National Academy of

Sciences. In 1950 he was elected an honorary life member of the Florida Soil Science Society.

Kelley was a Mason and a Congregationalist. He was a political liberal in the best sense. He participated to some degree in civic activities, especially those related to his special interests, but more or less steered clear of too much involvement in either outside civic organizations or committee work within the University.

In recognition of his achievements, he was elected a Fellow of three societies—the American Association for the Advancement of Science, the American Society of Agronomy, and the American Mineralogical Society.

In 1930 he was elected President of the American Society of Agronomy; earlier he had served as President of the Western Society of Soil Science, of which he was a founder and charter member.

He was elected to the National Academy of Sciences in 1943. In 1950 he was made Honorary President of the International Society of Soil Science, in recognition of his interest and assistance in helping to found this organization. Earlier (in 1935) he had been President of the American section of this organization and also President of its alkali section. His researches on alkali soils gained him wide recognition throughout the world, and he was frequently asked to serve as chairman of sections and to present invitational papers at meetings of international symposia. Such meetings were much less common then, in comparison to the current great increase in international travel and exchange.

Kelley also served as an associate editor and later honorary editor of *Soil Science*. He was an associate editor of *Agrochimica* (Italy) from 1950. After retirement in 1948, as mentioned earlier, Kelley was employed as a consultant with the Gulf Research and Development Company until 1955. He also acted

as a consultant to the U.S. Bureau of Reclamation for ten years (1948 to 1958).

In recognition of his contributions to the agriculture of California and his long and distinguished career as a scientist, Kelley was awarded an LL.D degree from the University of California in 1950. In 1958 the University of Kentucky awarded him a D.Sc. degree for the distinction and honor he had brought to his alma mater. This institution had elected him to Phi Beta Kappa during his student days. He was also elected to membership in the honorary societies of Sigma Xi and Phi Lambda Upsilon.

In his various positions, teaching students was not a part of his responsibilities. This is unfortunate, for with his energy, forceful personality, ability to think and speak clearly, fierce independence, and lively mind and spirit he would have generated enthusiasm and motivation, on the one hand, and insisted on high performance on the other. He believed strongly in basic research, though he fully recognized the importance of practical problems in producing motivation and direction.

His life was one of enthusiastic devotion to his science; his contributions to soil science and agriculture were many and distinguished; and his impact on colleagues, friends, and relatives was powerful and enduring.

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KEY TO ABBREVIATIONS

- Am. Chem. Soc. Monograph = American Chemical Society Monograph
 Am. Soil Survey Assoc. Bull. = American Soil Survey Association Bulletin
 Calif. Citrogr. = California Citrograph
 Centr. Bakteriöl., Parasitenk. u. Infektionskr. = Centralblatt für Bakteriologie, Parasitenkunde und Infektionskrankheiten
 Hawaii Agr. Exp. Sta. Bull. = Hawaii Agricultural Experiment Station Bulletin
 Hawaii Agr. Exp. Sta., Honolulu, Press Bull. = Hawaii Agricultural Experiment Station, Honolulu, Press Bulletin
 J. Agr. Res. = Journal of Agricultural Research
 J. Am. Chem. Soc. = Journal of the American Chemical Society
 J. Am. Soc. Agron. = Journal of the American Society of Agronomy
 J. Geol. = Journal of Geology
 J. Ind. Eng. Chem. = Journal of Industrial and Engineering Chemistry
 Proc. & Pap. 1st Internat. Congr. Soil Sci. = Proceedings and Papers of the First International Congress of Soil Science
 Proc. Soil Sci. Soc. Am. = Proceedings of the Soil Science Society of America
 Rept. Com. Sedimen. 1940-41, NRC, Washington, D.C. = Report of the Committee on Sedimentation 1940-41, National Research Council, Division of Geology and Geography, Washington, D.C.
 Soil Sci. = Soil Science
 Trans. 3d Internat. Congr. Soil Sci. = Transactions of the Third International Congress of Soil Science
 Univ. Calif. Agr. Exp. Sta. Bull. = University of California Agricultural Experiment Station, Bulletin
 Univ. Calif. Agr. Exp. Sta. Circ. = University of California Agricultural Experiment Station, Circular
 Univ. Calif. Agr. Exp. Sta. Hilg. = University of California Agricultural Experiment Station, Hilgardia
 Univ. Calif. Agr. Exp. Sta. Tech. Pap. = University of California Agricultural Experiment Station, Technical Paper

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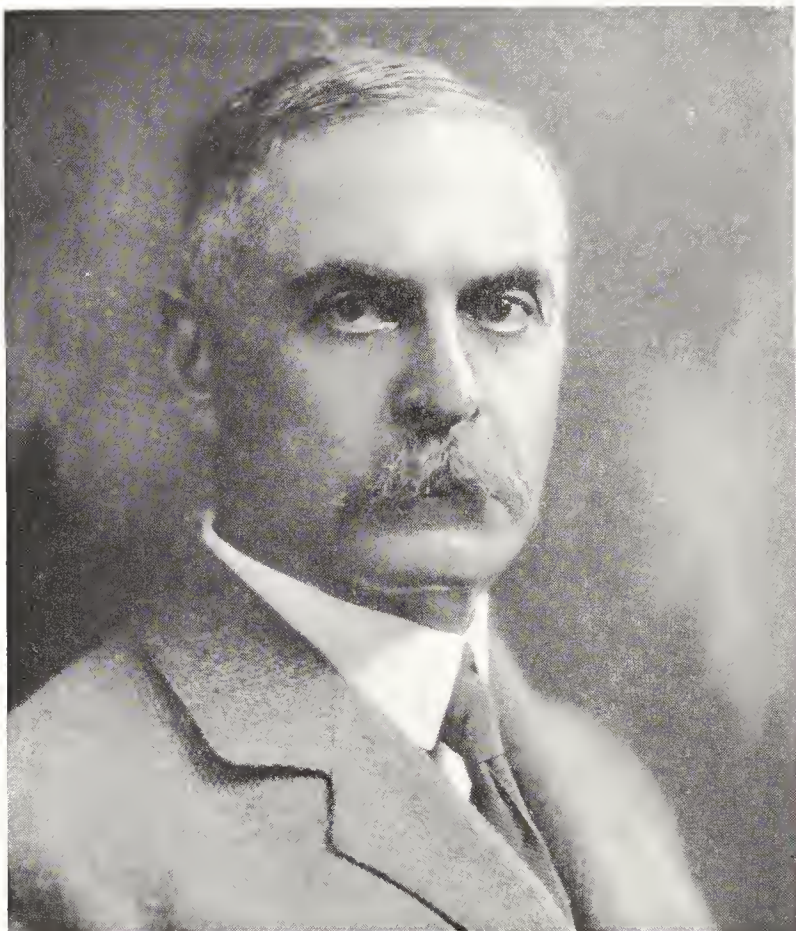
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H. Sandteimer

KARL LANDSTEINER

June 14, 1868–June 26, 1943

BY MICHAEL HEIDELBERGER

THE TIME IS PAST when one man can know all of science. Karl Landsteiner was one of the last possessed of the tremendous intellect that could comprehend and, better still, use practically all of the scientific knowledge of his time. He had been classified as a pathologist, but he studied organic chemistry with Bamberger, Hantzsch, and Emil Fischer and used his mastery of it to lay the groundwork of the chemical basis of immunological specificity, and he later read Max Planck's *Quantum Theory* and assimilated that, too.

Karl Landsteiner was born in or near Vienna on June 14, 1868, eleven years after the marriage of his parents, Fanny Hess and Dr. (of laws) Leopold Landsteiner, a well-known Austrian journalist and editor. When Karl was six his father died and some years later mother and son left the Jewish faith and became Roman Catholics. Karl's schooling began at the age of five and in later years he was always well up in the top half of his class. Entering medical school in 1885, he soon began experimental work in chemistry under the guidance of Ernst Ludwig, who was said to have been one of the most inspiring teachers of his time. In 1891 Landsteiner was awarded the degree of "Doktor der gesamten Heilkunde," surely a comprehensive title for a young man who had not quite reached his twenty-

third birthday. In the same year he functioned in the clinic of the University Hospital in Vienna and began several years of advanced study and research in organic chemistry under Emil Fischer in Würzburg, Eugen von Bamberger in Munich, and Arthur Hantzsch and Roland Scholl in Zurich. Not only were the results published in papers with three of these outstanding men, but the knowledge of organic chemistry and the feeling for it gained in these years guided Landsteiner through his entire career, as we shall see.

Until his twenty-seventh year Landsteiner also served in the University's first surgical clinic. On January 1, 1896, he was appointed assistant in the Institute of Hygiene of Professor Max von Gruber, and there, in spite of crowded conditions, began Landsteiner's interest in, and fundamental contributions to, immunology and serology. Nearly two years later he transferred to the Pathological-Anatomical Institute of Professor Anton Weichselbaum and worked there productively until the end of 1907. During this period he performed countless autopsies and pathological examinations, managing nevertheless to carry on far-ranging research on a variety of topics. This was published in seventy-five papers, many of which were fundamental additions to medical knowledge.

As early as 1900 Landsteiner became convinced that the hemagglutination of one blood by another was a physiological phenomenon and not pathological, as had been thought. Following extensive tests on the red blood corpuscles and sera of patients, colleagues, and himself, he developed the concept of the three blood-groups A, B, and C (now known as O). Thus began a revolution in the care and treatment of patients that eventually robbed blood transfusions of their principal dangers and ultimately won for Landsteiner the Nobel Prize in Medicine.

Shortly after his first publications on the subject Landsteiner showed the value of blood grouping in the identification of

bloodstains and in other medicolegal problems. However, there was a lag of years in the impetus to the increased use of transfusions of blood owing to the dangers and imperfect techniques which had previously made the procedure hazardous. It was not until 1907 that the first matching of bloods was carried out by Dr. Reuben Ottenberg of Mt. Sinai Hospital, New York, nor was the anticoagulant, sodium citrate, adopted until 1915. The principal problem today is to find enough donors for the rivers of human blood transfused. Studies on the chemistry of the blood-group substances have kept pace with the development of serological refinements and knowledge, and Landsteiner would certainly have rejoiced at the present state of the vast chapter of medicine and biochemistry for which he wrote the preface and laid down the fundamentals.

Other important discoveries were made by Landsteiner and his co-workers during his years in Weichselbaum's institute and these were mentioned in his "Habilitationen" request for the "Venia legendi" or right to lecture. Independently of Jules Bordet, he showed that injection of foreign red blood cells into an animal gave rise to hemolysins and hemagglutinins. During this period, also, he discovered, with Donath, cold agglutinins, and so cleared up the mechanism of paroxysmal hemoglobinuria. He was also among the first, if not actually the first, to dissociate the products of the interaction of antigens and antibodies and so to prepare partially purified antibodies. He was the first to use the technique of dark-field microscopy for the identification and study of the spirochetes of syphilis, the first to show that the Bordet-Wassermann test for syphilis could be carried out efficiently with "antigens" extracted from organs of normal animals as well as from syphilitic tissues, and the first to transmit syphilis to monkeys.

On January 1, 1908, Landsteiner became chief of pathology at the Wilhelmina Hospital in Vienna. During the same year, he

and E. Popper succeeded in transmitting poliomyelitis to monkeys and stated that the disease was apparently caused by a virus, thus laying the foundation for the further research that has resulted in the virtual elimination of this scourge. In 1911 Landsteiner received his first official recognition in an appointment to the Medical Faculty of the University of Vienna as Adjunct Professor without salary. During World War I he also served in War Hospital No. 1 and, in recognition of his services, was given the title of "Regierungsrat" in 1917. One year before, at the age of forty-eight, he married Helene Wlasto, who was twelve years his junior. A year later their only child, Ernst Karl, now a practicing physician in Providence, Rhode Island, was born.

At about this time, in spite of the difficulties caused by the war, Landsteiner began the studies which laid the foundation for the recognition of the chemical basis of immunological specificity. However, the lack of facilities for research, the civil disorders in Vienna, and the financial difficulties in making ends meet were so compelling that he transferred his activities to the Netherlands, where he was appointed "Prosektor" in a small Roman Catholic hospital at The Hague in 1919. The facilities and living conditions immediately after the war were better there than in Vienna, and while at The Hague Landsteiner began work on the immunology of hemoglobin and carried further the initial studies with azoproteins, to which further reference will be made. However, the uncertainties of his position made him receptive to an offer of a membership at the Rockefeller Institute in New York and this he decided to accept.

Karl Landsteiner and his wife and small son left their pleasant little house at the seaside in Scheveningen in the spring of 1923 and emigrated to New York, where Dr. Simon Flexner, Director of the then Rockefeller Institute for Medical Research, had invited Landsteiner to full membership, the highest rank in

the Institute. Dr. Peyton Rous tells how, when met at the ship and asked what sort of living quarters he would need, Landsteiner said: "A little cottage by the sea with a rose garden, such as I had in Scheveningen . . . at about \$50 a month." Needless to say, the flat ultimately selected represented a drastic compromise, and life in the big city was so different that Landsteiner often said that he wished he could lock his family in when he went out to work in the morning. However, readjustments were made and he became a citizen of the United States as soon as the law allowed.

As a man of broad interests and knowledge, Landsteiner was fascinated by the work his fellow-members were doing at the Rockefeller Institute and immediately enlisted the help of some of them in several joint projects. The longest-lasting of these was the study of the heterogenetic hapten with P. A. Levene, head of the biochemical division, but even this was broken off eventually in a clash of temperaments. Landsteiner, mild-mannered and sensitive as he was, nevertheless became unmistakably the "Herr Geheimrat" and absolute director in his laboratory and always insisted on running crucial tests himself. Quite understandably, this did not go well with other "Geheimrats," and experiments begun with Alexis Carrel and with James B. Murphy were not carried to conclusion.

Before arriving in the United States, Landsteiner had published several papers on the heterogenetic antigen, clarifying the apparently contradictory data of earlier workers. He found that an immunologically specific portion could be separated from the complex, and that this material, which had the properties of a lipid, did not by itself stimulate the formation of antibodies. Landsteiner adopted the term "hapten" for such substances. The heterogenetic hapten could, however, be converted into a complete antigen by simple admixture with proteins such as pig serum, itself a mixture, and this was considered a "Schlepper,"

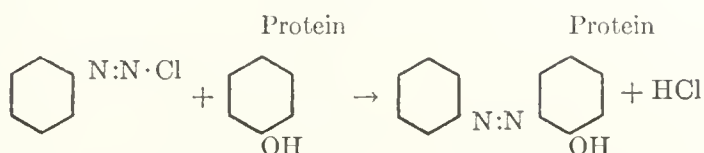
which "dragged" the hapten to the sites of formation of antibodies.

At the time Landsteiner arrived at the Institute I had developed a method in D. D. Van Slyke's laboratory in the Hospital of the Rockefeller Institute for the isolation of large quantities of crystalline horse oxyhemoglobin with an unimpaired capacity for release of, and combination with, oxygen. Landsteiner had previously studied the immunological properties of the hemoglobins of several species and he proposed that we inject horse oxyhemoglobin into rabbits and determine whether or not differences could be established between the oxyhemoglobins of different animal species. Conflicting answers to this question had been obtained by earlier workers. Landsteiner had just read Max Planck's book on the quantum theory and as a result suggested that, in addition to the serological tests, we run parallel physicochemical estimations of the solubility of each oxyhemoglobin in a saturated solution of the other. If there were no difference between the oxyhemoglobins, and if each were a single, pure substance, nothing more should dissolve. If, on the other hand, the oxyhemoglobins were chemically different, each should dissolve in a saturated solution of the other as in water alone. Tests along these lines showed clear differences between horse, dog, rat, and guinea pig oxyhemoglobins, while the oxyhemoglobins of the horse and donkey, two species which can be cross-bred, seemed identical or isomorphous.

Early in this century Pauly had shown that diazotized aromatic amines could be coupled to proteins and that colored products resulted in which the reaction had taken place mainly with the tyrosine and histidine residues. Obermayer and Pick used such azoproteins in attempts to alter the species specificity of proteins, but it was Landsteiner who realized the full implications of this reaction for immunology, seeing in it a means for modifying proteins in predictable ways by adding a great

variety of simple substances which might influence the specificity of the native protein used.

The principal chemical reaction is:



Depending upon the relative proportions of diazotized aromatic amine and protein, varying degrees of modification could be induced. If, after injection of the yellow or orange azoderivative into rabbits, a precipitate formed when antigen was added to the animal's serum, it was necessary to determine whether this was due to reaction with the hapten or added portion of the molecule, or whether the precipitation was simply one due to the carrier; that is, a protein-antiprotein interaction. Landsteiner and his co-workers solved this difficulty by coupling the same hapten to another protein unrelated to that injected and found that the sera of some of the rabbits still precipitated with the new antigen, showing that they had actually formed anti-hapten. A further confirmation was that an excess of the aromatic amine or its salt inhibited the precipitation, just as a large excess of antigen usually shifts the precipitin reaction (antigen-antibody precipitation) into a region of inhibition. In these ways it was found possible to distinguish between an *ortho*-, *meta*-, or *para*-substituent on the aromatic ring, particularly when the substituent was an acid group such as $-\text{COOH}$, $-\text{SO}_3\text{H}$, or $-\text{AsO}_3\text{H}_2$. All of this furnished a clear demonstration of the chemical basis of immunological specificity.

Additional phases of this work followed the discovery by Avery and myself that polysaccharides were the determinants of type-specificity in pneumococci, and paralleled experiments of Avery and Goebel showing that D-glucose and D-galactose and even α - and β -glucosides could be distinguished immunologically by means of antibodies elicited in rabbits by antigens

prepared by coupling the diazotized *para*-aminophenyl glycosides to proteins. Landsteiner and van der Scheer diazotized the *para*-aminobenzoyl derivatives of D-, L-, and DL-phenylaminoacetic acids and the amino-anilides of D-, L-, and meso-tartaric acids, coupled these to proteins, and showed that each stimulated the formation of distinctive antibodies. In this way immunological differences could clearly be shown between D- and L-isomers.

Landsteiner and van der Scheer also continued the former's earlier studies on the chemical and enzymatic degradation of proteins in an effort to solve the riddle of their specificities. With Landsteiner's characteristic thoroughness, they also began at the other end with peptides of known sequence. Glycylglycine, leucylglycine, glycylleucine, and leucylleucine were attached to proteins as haptens and it was found that the amino acid at the carboxy-terminal end exerted the stronger effect on specificity. With tri- and higher peptides of the same amino acids the results were less definite.

At about this time F. E. Kendall and I had developed methods for the quantitative measurement of many precipitating antibodies in units of weight. Landsteiner never made use of the method, contenting himself with reporting the results as +, ++, +++, etc. Whenever the writer visited Landsteiner's laboratory, Landsteiner would blushing apologize for not using the new method, and it was necessary to assure him that for his purposes a mass of quantitative data would add little to the often sharp qualitative distinctions his results disclosed. Like Jacques Loeb, who laid the foundations of modern protein chemistry, Landsteiner confined himself to the simplest methods capable of leading to an unequivocal conclusion and rightly avoided more laborious analytical techniques when simple qualitative tests sufficed.

Another time, when Landsteiner was returning a visit, I reached for a copy of his book to get it autographed and dis-

covered that it was missing. "Someone has stolen your book," I said. "That is the greatest compliment I ever received," said Landsteiner.

During the course of his epoch-making researches on the chemical basis of immunological specificity, Landsteiner carried forward two other major lines of work. One, with Philip Levine and A. S. Wiener, dealt with the chemistry and immunology of the blood-group factors. It added much to the knowledge of the human blood-factors M and N and resulted in the discovery of the Rh, or rhesus, factor, which many human beings share with rhesus monkeys and which was later shown to be responsible for the dreaded disease of infants, erythroblastosis fetalis. The blood-group factors of other primates were also studied and compared with those of the human species.

The second principal line of research, carried forward with John L. Jacobs and Merrill W. Chase, concerned itself with skin-sensitization and allergy. Simple chemical substances, such as chlorodinitrobenzene, which was known to produce industrial allergy, and picryl chloride, were shown to give rise to specific sensitization in guinea pigs. Experiments with other substances showed that only those capable of combining chemically with proteins could cause this effect, apparently reacting with one or more proteins of the skin or other tissues to form an antigen foreign to the animal. Similar principles were later shown to apply in man.

During his last years Landsteiner formed a warm friendship with Linus Pauling, and their discussions and arguments led Pauling not only to apply his great talents to immunology but also to contribute a chapter to the revised edition of Landsteiner's book, *The Specificity of Serological Reactions*.

Landsteiner became an Emeritus Member of the Rockefeller Institute in 1939, but continued working in his laboratory there with undiminished ardor. Although a sensitive musician and competent pianist, he gave up the piano in his later years. He

was, moreover, greatly upset by the events of World War II and was uneasy over the noise, the high incidence of crime, and the hurried pace of life in a great city. Finally, his wife fell ill of cancer, and her illness weighed heavily upon him. In the last week of June 1943 he suffered a coronary obstruction and entered the hospital of the Rockefeller Institute for treatment. Therapy was unavailing, and there he died.

The honors conferred upon Landsteiner were numerous. He was a member or honorary or foreign member, as the case might be, of the National Academy of Sciences (to which he was elected in 1932), the American Philosophical Society, the American Society of Naturalists, the American Association of Immunologists, Académie des Sciences of France, the New York Academy of Medicine, the Pathological Society of Philadelphia (as well as that of Great Britain and Ireland), the Royal Society of London, the Royal Society of Medicine, the Medical Chirurgical Society of Edinburgh, la Société Belge de Biologie, the Royal Danish Academy of Sciences, die deutsche Akademie der Naturforscher, l'Accademia Nazionale dei Lincei, the Royal Swedish Academy of Sciences, and the Swedish Medical Society, and was a Chevalier of the Légion d'Honneur. Landsteiner was awarded the Paul Ehrlich Medal and the Nobel Prize in Medicine in 1930, the Dutch Red Cross medal in 1933, and the Cameron Prize in 1938, and was given the degree of Doctor of Science, *honoris causa*, by the universities of Cambridge and Chicago, and by Harvard University.

IN COMPILING this biographical memoir I have been greatly aided by the more comprehensive publication, *Karl Landsteiner*, by Dr. Paul Speiser (Vienna, Hollinek Bros., 1961), and by the thoughtful review of Landsteiner's career and personality prepared by Dr. Peyton Rous for the Obituary Notices of Fellows of the Royal Society, Vol. 5, March, 1947. I am most grateful to Dr. Rous and Dr. Speiser for permission to make use of their material.

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KEY TO ABBREVIATIONS

- Ann. Inst. Pasteur = Annales de l'Institut Pasteur
 Ann. Rev. Biochem. = Annual Review of Biochemistry
 Arch. Derm. Syph. = Archiv fuer Dermatologie und Syphilis
 Beitr. chem. Physiol. u. Path. = Beitraege zur chemischen Physiologie und Pathologie
 Beitr. path. Anat. = Beitraege zur Pathologischen Anatomie und zur Allgemeinen Pathologie
 Ber. deut. chem. Gesell. = Berichte der deutschen chemischen Gesellschaft
 Biochem. Z. = Biochemische Zeitschrift
 Centr. Bakteriolog. Orig. = Centralblatt fuer Bakteriologie, Originale
 Centr. Bakteriolog. Ref. = Centralblatt fuer Bakteriologie, Referate
 Compt. Rend. Acad. Sci. = Comptes Rendus de l'Académie des Sciences (Paris)
 Compt. Rend. Soc. Biol. = Comptes Rendus des Séances de la Société de Biologie et de ses Filiales
 Deut. Z. ges. gerichtl. Med. = Deutsche Zeitschrift fuer die gesamte gerichtliche Medizin
 Jahresber. Ergebn. Immunitätsforsch. = Jahresbericht über die Ergebnisse der Immunitätsforschung
 J. Am. Med. Assoc. = Journal of the American Medical Association
 J. Exp. Med. = Journal of Experimental Medicine
 J. Immunol. = Journal of Immunology
 Klin. Wochschr. = Klinische Wochenschrift
 Med. Klin. = Medizinische Klinik
 Münch. med. Wochschr. = Münchener medizinische Wochenschrift
 Oesterr. Sanitätswesen = Oesterreichische Sanitätswesen
 Proc. Koninkl. Akad. Wetenschap. = Koninklijke Akademie van Wetenschappen te Amsterdam, Proceedings of the section of sciences
 Proc. Soc. Exp. Biol. Med. = Proceedings of the Society for Experimental Biology and Medicine
 Schweiz. med. Wochschr. = Schweizerische medizinische Wochenschrift
 Sitzber. k. Akad. Wiss. = Sitzungsberichte der Königliche Akademie der Wissenschaften, Mathematisch-naturwissenschaftliche Klasse, Wien
 Verhandl. 9. Congr. deut. dermat. Gesell. = Verhandlungen des neunten Kongress der deutschen dermatologische Gesellschaft
 Verslag Koninkl. Akad. Wetenschap. = Koninklijke Akademie van Wetenschappen te Amsterdam, Verslag van de Gewone Vergadering der Afdeling Natuurkunde
 Wien. klin. Rundschau = Wiener klinische Rundschau
 Wien. klin. Wochschr. = Wiener klinische Wochenschrift
 Wien. med. Wochschr. = Wiener medizinische Wochenschrift

- Zentr. allgem. Path. Path. Anat. = Zentralblatt fuer allgemeine Pathologie und Pathologische Anatomie
 Zentr. Gynäkol. = Zentralblatt fuer Gynäkologie
 Zentr. Physiol. = Zentralblatt fuer Physiologie
 Z. Hyg. Infektionskrankh. = Zeitschrift fuer Hygiene und Infektionskrankheiten
 Z. Immunitätsforsch. Orig. = Zeitschrift fuer Immunitätsforschung und Experimentelle Therapie Originale
 Z. klin. Med. = Zeitschrift fuer klinische Medizin
 Z. physiol. Chem. = Zeitschrift fuer physiologische Chemie

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CLARK BLANCHARD MILLIKAN

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BY E. E. SECHLER

CLARK BLANCHARD MILLIKAN was born in Chicago on August 23, 1903, almost simultaneously with the decision of the Wright Brothers that their airplane was ready to fly under its own power. The brothers shipped their frail craft from Dayton to Kitty Hawk in September of that year. Whether or not this coincidence was important can never be known, but it is a fact that, in his early impressionable years, Clark was surrounded by the publicity attendant on the rapid development of this new means of transportation.

Being the son of Robert Andrews Millikan, at that time Professor of Physics at the University of Chicago, Clark was sure to develop any latent interest in the new advances in science and technology. According to his own memory, Clark was only eight or nine years old when he first decided to make his life's career in some area associated with aeronautics. During this eight- or nine-year period many firsts in aviation were accomplished. After the first Wright Brothers' flight in December 1903, news announcements of startling events, both in the United States and abroad, were a regular occurrence. The first Aero Exhibition in America was held in New York in 1906 and the first airplane was ordered from the Wright Brothers by the Army Signal Corps in 1908 for a price of \$25,000.

The first transcontinental flight was made in 1911, and in 1912 Congress appropriated \$125,000 for Army aeronautics. All of these events would surely have been discussed at the home of a university professor, and it is little wonder that one of his sons saw glamorous possibilities in aeronautics as a future career.

Clark attended the University of Chicago Elementary and High Schools, and during this time he continued his interest in aeronautics by building model airplanes. It is unfortunate that none of these exist today for it would be fascinating to compare them with the complex machines that he was concerned with during the last years of his life. While in elementary and high school Clark found an outlet for his intense physical energy in athletics.

After spending a year divided between the University of California (Berkeley) and Throop Institute of Technology (now the California Institute of Technology), he entered Yale as a freshman in the fall of 1920. The decade between 1910 and 1920 really saw aviation technology and the science of aeronautics become accepted. The altitude record had risen from 9,700 feet to over 38,000 feet and Congressional appropriations for aeronautics reached nearly \$18 million in 1916, the year before World War I. Although Clark specialized in physics and mathematics at Yale, it is obvious that the impact of the airplane on the world conflict between 1917 and 1919 must have left a strong impression on his mind. At that time very few schools had courses in aeronautics, although Jerome Hunsaker was detailed (from the Navy) to Massachusetts Institute of Technology in 1913 to develop courses in aerodynamics.

While at Yale, Clark continued his interest in athletics, specializing in running the hurdles for the track team. In addition, he found a new avocation in music and sang in the Yale Glee Club. By the end of his undergraduate years he had

firmly established the pattern of his future life. Its major facets consisted of solid strength in the fundamentals of mathematics and physics, an intense interest in all phases of aeronautics, a love of music both as a participant and as a listener, and a physical energy that he enjoyed expending out of doors in some form of athletic activity. Binding all this together was an outgoing personality which made him many friends from all walks of life. Alive, vibrant, dynamic, and friendly were the adjectives which described Clark Millikan to those who knew him best.

He entered the Graduate School at Caltech in the fall of 1924 and continued to specialize in mathematics and physics. Although there were no formal courses in aeronautics at that time, Professor Harry Bateman, the distinguished mathematician and physicist, had a strong interest in theoretical aerodynamics. It was under Professor Bateman that Clark did his doctoral dissertation on "The Steady Motion of Viscous Incompressible Fluids." He received his Ph.D. in 1928. His interest in all phases of aeronautics remained high, as evidenced by the fact that he and two colleagues, Arthur L. Klein and Albert Merrill, designed, built, and flew a revolutionary new type of biplane in which control was established by moving the complete biplane wing assembly. The placement of the two wings was such as to develop a built-in stability, and this airplane, fondly named the "Dill Pickle," could be flown "hands off," an unusually daring feat in those days.

Throughout the 1920s there was an ever-growing activity in aviation in Southern California. By 1926, when the Loughhead Brothers started what was to become the Lockheed Aircraft Company in nearby Burbank, Douglas was already a thriving company in Santa Monica and the whole Los Angeles area was becoming aviation-minded. Small firms were springing up from Burbank to San Diego and, since Caltech was one

of the leading engineering schools in Southern California, it was natural that there should be an early and continuous contact between this new and exciting industry and the staff and students of Caltech. Donald Douglas, Sr., and his chief engineer, Arthur Raymond; "Dutch" Kindleberger, who went from Douglas to North American; "Kelly" Johnson, from Lockheed; and Jack Northrop were all friends of Caltech and soon became close personal friends of Clark Millikan. Through them he saw how aircraft were built and flown, thus rounding out his theoretical knowledge of the subject.

Another movement of importance was taking place during this period which had its influence in his life. In 1925 Daniel Guggenheim donated \$500,000 toward the development of a School of Aeronautics at New York University. At about the same time the Daniel Guggenheim Foundation for the Promotion of Aeronautics was established. From this Foundation Caltech was granted funds in 1928 to establish the Daniel Guggenheim Graduate School of Aeronautics and to build the Guggenheim Aeronautical Laboratory. The Fund also arranged for a visit of Dr. Theodore von Kármán to the United States from Aachen so that he could participate in the plans for the laboratory. Although the contribution of von Kármán was great, it was actually Clark Millikan and Arthur Klein who did the detail design work on the laboratory and its major piece of research equipment, a Gottingen-type, closed-return wind tunnel with a working section ten feet in diameter.

This wind tunnel, which had for that time the very high velocity of 200 m. p. h., was operated, calibrated, and turned into a highly efficient research tool largely through the efforts of Clark Millikan, "Maj." Klein, and a handful of graduate students in this embryo Aeronautics Department at Caltech. It was during this period that the acronym GALCIT (standing for the Guggenheim Aeronautical Laboratory of the Cali-

fornia Institute of Technology) was coined, and ever since the words GALCIT and Clark Millikan have been synonymous.

GALCIT was under the directorship of von Kármán from 1930 until 1949. During that time Clark was in charge of the Applied Aerodynamics phases of the laboratory's activities and supervised all of the testing and research carried out in the wind tunnel. Since this was the only available large wind tunnel in Southern California, the local, and sometimes some distant, aircraft companies found it an ideal piece of test equipment for developing new aircraft designs. The entire DC-series of Douglas airplanes and many of the early Boeing airplanes, as well as those of North American Aviation, Northrop, Lockheed, and Consolidated, were put through their aerodynamic paces in the GALCIT wind tunnel. This was always done under the watchful, critical, and analytical eyes of Clark Millikan. In fact, in the early days he could often be found performing manual labor as part of the test-operating crew just because he liked to be doing something useful.

Through these contacts he had a very significant influence on the early development of many of the important airplanes of the 1930s and 1940s. Concurrently with this activity, he was also an excellent teacher, and he rose from Assistant Professor in 1928 to Associate Professor in 1934 and to full Professor in 1940. He always prepared his lectures meticulously and, because his classes knew he performed many experiments in aerodynamics himself and was thoroughly acquainted with all the new developments in the field, they knew they were getting the latest and the best information.

In addition to his more than full academic life he also took a leading role in the activities of the Institute of Aeronautical Sciences (later the American Institute for Aeronautics and Astronautics) and was its president in 1937. He was made an Honorary Fellow of that society and was also a Fellow of the

Royal Aeronautical Society of Great Britain, the American Academy of Arts and Sciences, and the American Physical Society, as well as a member of the American Association for the Advancement of Science and the honorary societies of Phi Beta Kappa, Sigma Xi, and Tau Beta Pi. Unlike many people in the engineering and scientific world who join societies but take no part in them, Clark Millikan was not only a member of the above organizations but was usually an "active" member in the truest sense of the word. He served as an officer on executive councils and advisory boards, and his advice and participation were not only solicited but were given generously. In this manner, his influence extended far beyond the confines of his already broad academic activity at Caltech.

Over and above all of this he had time to be a person. He loved, enjoyed, and appreciated his family. He made friends easily and had many of them all over the world. All forms of outdoor activity appealed to him, and one of his favorite pastimes consisted of taking trips to the out-of-the-way back country in Southern California along with friends who were *simpatico* with rough modes of travel. Borrego Springs and Painted Canyon, near where his ashes have been scattered, were spots he knew long before they became popular to the general public. In fact, when he used to go into some of the desert areas, he was forced to take along strips of canvas which he would use as a makeshift roadway over some of the more difficult sandy spots. The Navaho country was also one of his favorite vacation areas and the more isolated the region was, the better he loved it. In later years he enjoyed the redwoods of the Bohemian Grove along the Russian River in California and he always returned from a short stay in this spot with renewed physical and mental energy.

Another indication of his affection for people could be found in his club memberships. He belonged to and took an

active part in the activities of the California, Bohemian, Sunset, and Twilight clubs as well as the Yale Club in New York. In all of these he was not just a name on a membership list but was on a first-name basis with nearly all of the people belonging to these organizations.

With all of Clark's outside activities his colleagues were constantly amazed at the impact that he could continue to have on Caltech and the Jet Propulsion Laboratory. He authored approximately forty technical papers as well as the first volume of the GALCIT Aeronautical Series of Textbooks. This book was entitled *Aerodynamics of the Airplane* and was an outgrowth of a course he taught to his graduate students.

During its fifteen-year existence (1945-1960) he was Director of the Southern California Cooperative Wind Tunnel and contributed greatly to its success. This was a joint venture financed by five Southern California aircraft companies and managed and operated by Caltech. It was one of the first large supersonic wind tunnels and was known throughout the world for its efficiency, flexibility, and accuracy. It contributed greatly to the development of postwar commercial and military aircraft, and a large part of this contribution came about because of the intense interest of its Director in every phase of the operation and in the aerodynamic phenomena being studied. One incident, which was typical, shows that Clark Millikan was a real participant in the activities of the wind Tunnel. During the calibration stages he insisted on personally checking the flow patterns while the tunnel was operating. One day, while working in the region just ahead of the working section, he got too close to the contraction section where the velocity started to increase and he was suddenly blown into, through, and then out of the high-speed working section of the tunnel. His deep personal interest in the mechanical and technical, as well as the administrative, aspects of the project was one of

the reasons why personnel morale was exceptionally high in this organization.

His contributions to the military strength of the United States were great. With his wide knowledge of the entire aeronautics field and his personal friendship with so many of its leaders, his services and advice were constantly being sought by—and, what is more, were generously given to—the Armed Services. Although his later activity in the field of missiles and spacecraft was largely connected with the Air Force through his membership on its Scientific Advisory Board (from 1952 until his death), he also had contact with other services. He was a Lieutenant Commander (USNR) from 1942 to 1946; served on the Naval Research Advisory Committee from 1947 to 1950; was a member of the Defense Science Board of the DOD (1957 to 1962); and was chairman of the Guided Missile Committee of the Research and Development Board of the Pentagon. He was influential in Army circles through his membership on the Army Ballistic Research Laboratory's Scientific Advisory Committee.

Both the National Advisory Committee for Aeronautics and its successor the National Aeronautics and Space Administration made use of his knowledge by appointing him to advisory committees and councils. As a result of his activities in both civilian and military aeronautics he was awarded the United States Medal for Merit, the United States Air Force Exceptional Service Award, and the King's Medal for Service in the Cause of Freedom of Great Britain. His eminence in his field was also recognized by membership in the National Academy of Sciences (1964) and the recently formed National Academy of Engineering (founding member, 1964).

The area around Pasadena also felt his influence. He, along with von Kármán and colleagues, started the Jet Propulsion Laboratory, which at one time was a small research group supported by the U.S. Army and connected directly to GALCIT.

Its growth is well known both before and after it became a part of NASA. Clark served on its advisory board from the beginning and was always trying to develop a closer liaison between the activities of JPL and those of Caltech. He was instrumental in the formation of the Aerojet-General Corporation and served on its Board of Directors as well as on its Technical Advisory Board. He was a member of the Board of Directors of the National Engineering Science Company in Pasadena.

From the time of his graduation with a doctor's degree in 1928 until early 1950 the development of both civilian and military aviation is a matter of history. However, few people who use this mode of transportation for pleasure, business, or warfare ever realize the influence played on their lives by this Professor of Aeronautics. Although he wrote a number of papers, they were generally not deeply scientific in nature. Clark Millikan's influence was through two other major channels. One was his personal contact with nearly everyone who was important in aeronautics. He could exchange ideas with these people and thereby synthesize an over-all picture of the problems and the progress of the engineering and scientific discipline. The other channel was his influence on the students who were trained at and graduated from Caltech.

Following the basic plan established by von Kármán, his policy for GALCIT (Clark became its Director in 1949) was to obtain the best teaching staff in the various aeronautical disciplines, give them the best possible research equipment, pick a small number of carefully selected graduate students, and then turn out a product mix of graduates and research that was second to none in the world. In addition to his interest in the operation of the subsonic wind tunnels in GALCIT, he also had a personal interest in the research in hypersonic flow being carried out in the laboratory. Even with his heavy outside work load he insisted on teaching an advanced course in the theory of real fluids. Just before his death he had com-

pleted a manuscript for a text on this subject which is now being prepared for publication.

His impact on students was both nation- and world-wide. Each year, students from both the Air Force and the Navy were in his classes, as were students from many other nations. He always insisted on knowing these students personally, and the parties he gave in his home in the hills above Pasadena will always be remembered as a welcome relief from the rigors of the Caltech program. Gathered around the piano (he was a good pianist—and his memory of the verses of old drinking songs was fabulous), the students, as well as important guests, would be introduced to the other side of Clark, the side that was warm, home-loving, and that enjoyed good food, good drink, and good friends.

It was in this way that he became acquainted with young men who later became top administrators in aircraft companies or department heads in important educational institutions, and some who became Naval admirals and Air Force generals, along with others who returned to their own countries to take important positions in research, industry, and teaching. His influence on their thinking and their actions is impossible to document, but many of them have said it was great.

In the early 1950s a new facet of aeronautics came into view and Clark received its challenges with his usual enthusiasm. The long-range missile and the concept of satellites and spacecraft became possibilities, but only to men of vision. Of these, one was Clark Millikan. Obviously the Air Force Scientific Advisory Board was in the forefront of the activity and Clark knew many of those in the Ballistic Systems Division who were encouraging much of the new thinking on such subjects. As with everything he had done previously, he became an enthusiastic supporter of these new efforts and was generous of time and talents to those who needed them.

At Caltech, new ideas had to be injected into courses and research programs under his guidance. In order to take care of the new programs the original Guggenheim Laboratory was supplemented by the Firestone Laboratory of Flight Sciences and the Kármán Laboratory of Fluid Mechanics and Jet Propulsion. This new complex retained the GALCIT name but it now referred to the Graduate Aeronautical Laboratories. The programs at JPL had to be rearranged and updated and his activities connected with their programs increased.

Even though he had added these extra burdens, he continued all his other interests in music, club and professional society functions, and civic affairs. He became a member of the Los Angeles Committee on Foreign Relations, an affiliate of the Foreign Affairs Council in New York. In the 1960 presidential election campaign he became active in helping to define the place of the engineer and scientist in national politics.

Clark Millikan never knew how to spare himself and, even when ill, continued a work load that would have been impossible for many men in good health. This refusal to stop may have hastened his passing, but he simply could not live in any other way. If he had a major fault this was it, but it was far overshadowed by his contributions to aeronautics and to society. He worked under a double handicap in that he was the son of the great physicist Robert Millikan and taught in the school his father had made famous. But his achievements and contributions and his place in the annals of aeronautics were his alone and were accomplished by a work-and-play program that set new records for what could be accomplished in a given time. From the first 120-foot flight of the Wright Brothers to men in orbit and interplanetary probes is a distance that few minds can completely grasp. Clark Millikan not only understood this development but contributed to nearly every step along the way.

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J. Aeron. Sci. = Journal of the Aeronautical Sciences

NACA Tech. Rept. = Technical Report of the National Advisory Committee for Aeronautics

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E. O. Murphy

EGER VAUGHAN MURPHREE

November 3, 1898–October 29, 1962

BY EDWIN R. GILLILAND

EGER V. MURPHREE was a pioneer in the introduction of the newly developing chemical engineering science into industry and he was an intellectual and administrative leader in changing petroleum processing from a mechanical to a chemical industry.

Eger Vaughan Murphree was born November 3, 1898, in Bayonne, New Jersey. When he was eight years old his parents moved to Louisville, Kentucky, where he attended Louisville Mayo High School preparatory to enrolling at Kentucky University. At Kentucky he majored in chemistry and mathematics. In addition to his scholastic activities he was active in athletics. He was captain of the football team in 1920, and was rated All-Southern tackle. Following his graduation from Kentucky in 1920 he remained another year for graduate work in chemistry and obtained an M.S. degree in 1921. Combining his interest in science and football, he next accepted a position as physics and mathematics instructor and football coach at Paris High School, Paris, Illinois.

The following year he went to the Massachusetts Institute of Technology and was a Staff Assistant and Research Associate in the Chemical Engineering Department.

This was an exciting time in chemical engineering at M.I.T.

Dr. W. H. Walker had outlined the modern type of chemical engineering education and he had found in Dr. W. K. Lewis a man to develop this program. At the time Murphree arrived in Cambridge, the Chemical Engineering Department was brimming with enthusiasm in the study of the mechanism of the physical and chemical interreactions occurring at the interface separating solids, liquids, and gases. It did not take Lewis long to recognize that Murphree was a man of unusual technical ability and promise. He was thoroughly grounded in physics and chemistry. He had a mastery of mathematics that went far beyond the qualifications of his contemporaries in the field of applied chemistry. He had a well-developed scientific imagination. He combined with all this an unusual sense of the relevant and the practical. Very soon he became an important member of the Department team. At M.I.T. Murphree started to use the rate of solution as a technique in studying the effect of various methods of agitation on interreactions between solids and liquids. Soon, however, his interest was directed to the problems of distillation and rectification, and his work in this field throws a flood of light on the character of his mind.

The quantitative approach to engineering is rarely purely empirical. However, ultimate relationships are almost always extremely complicated. In the field of distillation the methods of the past had assumed that the vapor rising through the liquid on the plate of a rectifying column would come to complete equilibrium. Failure to obtain equilibrium in the column was allowed for by insertion of a correction coefficient, a so-called plate efficiency. In many situations this approximation was satisfactory but in important cases it led to gross confusion. Murphree had this impressed upon him when faced with the engineering problems of the complex reaction in ammonia recovery towers of the Solvay process, involving the three important volatile components, ammonia, carbon dioxide,

and water. He recognized that in these multicomponent systems the old approach was worse than sterile, it was misleading. He started new thinking by focusing attention upon what happens progressively to the bubble of vapor as it rises through the liquid on the plate, endeavoring thus to develop a better understanding of factors governing plate efficiency. This approach immediately threw helpful light on what was going on in the column. It is true that it involved more complicated methods of computation but Murphree showed how to handle these. He was the first to study constructively the mechanisms of interactions between vapor and liquid in the process of distillation.

Murphree was a master at hitting a sound balance between the completeness of analysis and the limitations inherent in the computation. In the use of mathematics he combined imagination with practicality. He put his analysis of the distillation system into useful form for other engineers, and the Murphree plate efficiency is the most widely used today in distillation calculations.

Murphree also studied the fundamental problems of turbulence at the interface between a liquid and a solid. This problem had been attacked by Prandtl through the assumption of the existence of a lamellar film at the interface, separating the interface itself from the main body of the fluid moving past it. The Prandtl approach was equivalent to assuming a discontinuity between the surface film and the main liquid body in that it neglected mixing, other than molecular diffusion in the film. Murphree, recognizing the improbability of any such discontinuity, developed a theory of progressive increase in turbulent motion as one moves out into the fluid and proceeded to apply the analysis to a study of the interrelationships of fluid friction and heat transfer. This work was interrupted when he left the Institute for an industrial

career. The approach proposed by Murphree is receiving more and more attention because it clearly comes closer to reality than the picture of discrete layers.

Murphree accomplished these major contributions in two years at M.I.T. In 1924 he left to accept a position as Chemical Engineer with the Solvay Process Company, but his major industrial career began in 1930 when he left Solvay to accept a position as Director of a new group being formed by the Standard Oil Company (New Jersey) to develop chemical processes utilizing petroleum material. He progressed rapidly and in 1947 he was elected President of the Standard Oil Development Company.

Murphree's leadership of the research and development work of the Standard Oil Company (New Jersey) led to many important developments, a number of which were of great significance to the country during World War II. Four examples will illustrate the ability of this group under his leadership to take chemical reactions developed in the laboratory to commercial-plant operation in a minimum of time.

When World War II broke out the only American source of toluene was from coal tar, and its availability was seriously limited. Murphree's organization had studied the synthesis of toluene from petroleum constituents, and fortunately a group of plants of associated and affiliated companies had the facilities and the organization to test out the individual steps in commercial equipment. The dramatic story of this pilot plant test, in operations extending from Texas to New Jersey to Louisiana, is matched by that of the construction in a little over a year by a Texas affiliate, the Humble Oil and Refining Company, of a commercial plant which in the first year of American participation in the war supplied two thirds of the toluene used in explosives.

The initiation of the synthetic rubber program required

large supplies of butadiene. Its initial production from alcohol saved the day in the early stages of the program, but it was clear from the start that this monomer must ultimately come from petroleum. Murphree's organization had developed in the laboratory a new catalyst and a new technique of operating control for butadiene synthesis by the dehydrogenation of butenes. From the data available he reached the conclusion that in view of the crisis it was justifiable to bypass the usual pilot plant stages of development. He convinced not only his organization but the Rubber Director's Office and the staff of other companies as well. The successful completion and operation of fourteen plants proved his technical judgment right.

A similar situation arose in the development of butyl rubber, so promising for inner tubes because of its low permeability to air. The process had been developed in the laboratories of the Standard Oil Company (New Jersey) and Murphree decided that the process was technically sound and that a short circuiting of the development stages was justified. The plants were built and successfully operated.

The development by the Standard Oil Company of the fluid catalyst technique for the cracking of oil made a major contribution to the war effort. To obtain high performance from the reciprocating engines used in the airplanes during the war it was necessary to have high octane gasoline that could be made satisfactorily only with catalytic cracking. Murphree's organization developed a continuous process for the reaction by the fluidized solid technique. There were many problems to be solved in going to the tremendous-sized reactors needed. These were chemical reactors much larger than had ever been built for any reaction and they used a catalyst that had to be regenerated every few minutes. There were many engineering obstacles which had to be overcome and Murphree's

guidance and judgment were crucial in many stages. The demand for gasoline was so great that the construction of thirty-two fluid catalytic cracking plants was underway before the first commercial plant was in operation, but Murphree had concluded that the process was sound on the basis of small-scale tests and was bold enough to proceed. The successful operation of these plants demonstrated the soundness of his judgment.

In addition to the work he did for the war effort through his organization, he also served as a member of the Office of Scientific Research and Development, S-1 Executive Committee headed by Dr. J. B. Conant. The committee was instrumental in the establishment of the Manhattan project, which developed the atomic bomb. Prior to the organization of that committee, Murphree was chairman of the Planning Board, which was responsible for the technical and engineering aspects of the procurement of materials for the construction of the pilot and full-size production plants. In addition, Murphree was in charge of the design of the heavy water plant in British Columbia and supervised much of the work on the centrifugal method of separating uranium isotopes.

Murphree will be long remembered for the many contributions he made to the development of the process industries, both for specific technical contributions and for his organization of the principles and the methods of attacking the industrial application of laboratory results.

HONORS AND DISTINCTIONS

MEMBERSHIPS

Academy of Political Sciences
 American Chemical Society
 American Nuclear Society (Charter Member)
 American Institute of Chemical Engineers
 American Institute of Mining and Metallurgical Engineers
 American Institute of Physics
 American Petroleum Institute
 American Physical Society
 Armed Forces Chemical Association
 Directors of Industrial Research
 Industrial Research Institute
 Institute of Petroleum
 National Academy of Sciences (1950)
 New York Academy of Sciences (Fellow, 1955)
 Society of Chemical Industry

SPECIAL SERVICES

Office of Scientific Research & Development, S-1 Section, 1941-1942;
 S-1 Executive Committee, 1941-1945
 Chairman, Planning Board (D.S.M.) Project, 1941-1942
 Manhattan District Project, 1942-1945
 General Advisory Committee of the Atomic Energy Commission
 Atomic Energy Commission Ad Hoc Advisory Committee on Reactor Policies and Programs, 1958-1961
 Advisory Panel on Atomic Energy, Office of Assistant Secretary of Defense, 1958
 U.S. Chamber of Commerce Committee on Commercial Uses of Atomic Energy, 1960-1961
 Army Scientific Advisory Panel on Atomic Energy, Department of the Army
 Special Assistant to Secretary of Defense for Guided Missiles, 1956-1957
 Finance & Budget Committee, Atomic Industrial Forum
 Permanent Council of the World Petroleum Congress, 1949-1962;
 Chairman, 1951-1959

President of Fifth World Petroleum Congress, 1956-1960

Chairman of U.S. National Committee for Sixth World Petroleum Congress, Inc.

Visiting Committee, Department of Chemistry, Harvard University, 1955-1956

Visiting Committee, Department of Chemistry, Massachusetts Institute of Technology

Kentucky Research Foundation

HONORS

Perkins Medal for work in applied chemistry, American Section of the Society of Chemical Industry, 1950

Industrial Research Institute Award for outstanding accomplishments in the organization and direction of research, 1953

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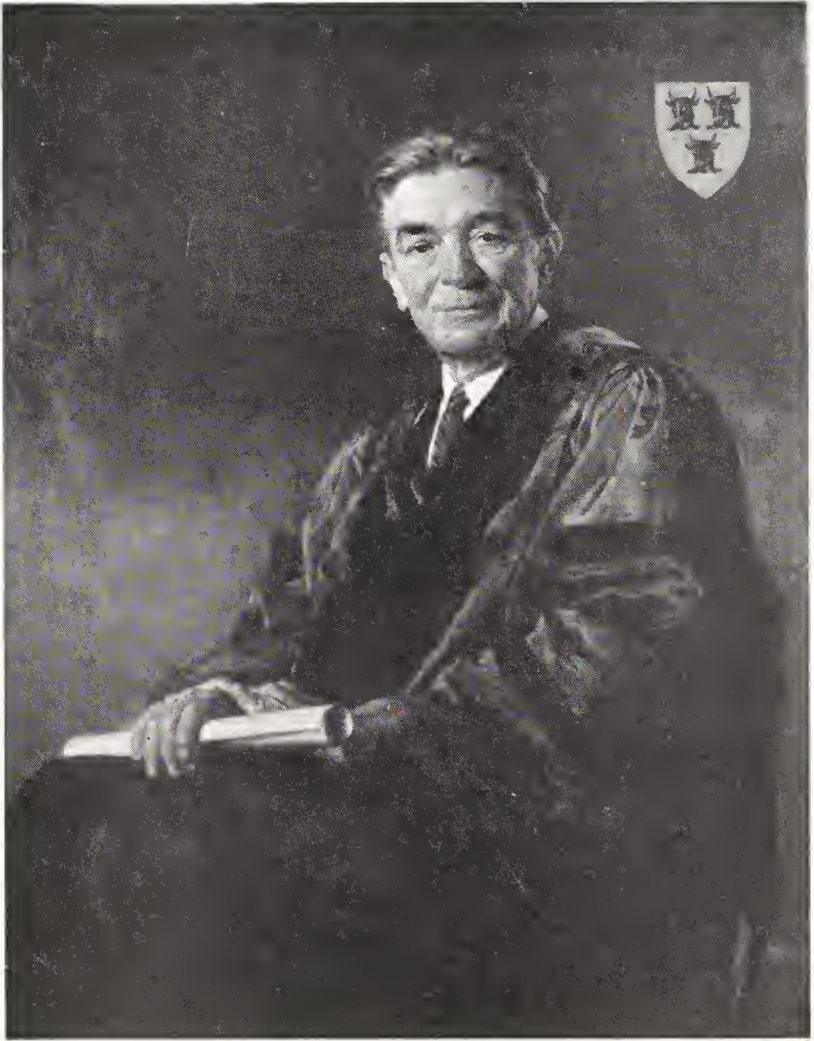
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- 2,156,266 Obtaining Oxidation Products from Hydrocarbons
- 2,165,807 Separation of Mixtures of Liquid and Solid Constituents
- 2,183,148 Purifying Acetylene Gas and Manufacturing Acetaldehyde
Therewith
- 2,191,675 Solid Lubricant Composition
- 2,199,837 Catalyst Regeneration
- 2,227,149 Lubricating Oil for Diesel Engine
- 2,256,621 Refining Process
- 2,256,622 Manufacture of Hydrocarbon Products by Hydrogenation of
Carbon Oxides
- 2,311,318 Process for Cracking Oils
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J. D. Whitaker.

JOHN SPANGLER NICHOLAS

March 10, 1895–September 11, 1963

BY JANE M. OPPENHEIMER

There is a line among the fragments of the Greek poet Archilochus which says: "The fox knows many things, but the hedgehog knows one big thing." Scholars have differed about the correct interpretation of these dark words, which may mean no more than that the fox, for all his cunning, is defeated by the hedgehog's one defense. But, taken figuratively, the words can be made to yield a sense in which they mark one of the deepest differences which divide writers and thinkers, and, it may be, human beings in general. For there exists a great chasm between those, on one side, who relate everything to a single central vision, one system less or more coherent or articulate, in terms of which they understand, think and feel—a single, universal, organizing principle in terms of which alone all that they are and say has significance—and, on the other side, those who pursue many ends, often unrelated and even contradictory, connected, if at all, only in some *de facto* way, for some psychological or physiological cause, related by no moral or aesthetic principle; these last lead lives, perform acts, and entertain ideas that are centrifugal rather than centripetal, their thought is scattered or diffused, moving on many levels, seizing upon the essence of a vast variety of experiences and objects for what they are in themselves, without, consciously or unconsciously, seeking to fit them into, or exclude them from, any one unchanging, all-embracing, sometimes self-contradictory and incomplete, at times fanatical, unitary inner vision. The first kind of intellectual and artistic personality belongs to the hedgehogs, the second to the foxes; and without insisting on a rigid classification, we may,

without too much fear of contradiction, say that, in this sense, Dante belongs to the first category, Shakespeare to the second; Plato, Lucretius, Pascal, Hegel, Dostoevsky, Nietzsche, Ibsen, Proust are, in varying degrees, hedgehogs; Herodotus, Aristotle, Montaigne, Erasmus, Molière, Goethe, Pushkin, Balzac, Joyce are foxes.

Isaiah Berlin, *The Hedgehog and the Fox*.¹

IT WOULD BE inappropriate in the extreme to compare Nicholas' contributions with those of Goethe, Pushkin, and Balzac; he was simply one of those "human beings in general," to whom Berlin refers in the above quotation, who lead centrifugal lives. But he was extraordinary, too, in his own way. Highly versatile and original, he was endowed with stores of energy, intellectual and physical, that enabled him to make an unusually large and varied number of contributions to biology, both as an investigator and as a teacher. He was productive not only as a scientist; he served Yale in a variety of educational capacities during the greater part of his life, and performed important administrative services for both the University and the government for many years. And his life was many-faceted in other ways also, as we shall see.

Even the most sanguine of molecular biologists (if molecular biologists permit themselves to be described in humoral terms) would not suppose today that the base order in DNA codes accounts directly for attributes of personality. But Nicholas' parents were also both unusually energetic and vigorous, with great drive, powers of organization, and intelligence, and we may assume that one way or another, through heredity or environment, these factors influenced the later professional development of their son. His reasons for becoming a scientist will become apparent later, but meantime let us look more

¹ Cited from *The Hedgehog and the Fox*, by Isaiah Berlin, Simon and Schuster, Inc.; copyright 1953 by Isaiah Berlin.

closely into his life and his background during his early and formative years.

Members of the Nicholas family first came to America as Hessian mercenaries, but abandoned the Loyalist cause to join von Kalb's forces. Our Nicholas' father, the Reverend Samuel Trauger Nicholas, the son of a truck farmer, was born in Kintnersville, Pennsylvania, a few miles south of Easton, still a sleepy but lovely village on the Delaware River and the Delaware Division Canal. He graduated from Pennsylvania (now Gettysburg) College in 1890 and from the Gettysburg Lutheran Theological Seminary in 1893; he was later to become a Doctor of Divinity *honoris causa*, and he advanced far in his profession as a Lutheran minister. Nicholas' mother, née Elizabeth Ellen Spangler, born in Arendtsville, Pennsylvania, was the daughter of the proprietor of the village store. The Spanglers, too, are an old family in Pennsylvania; Spangler's Spring, a landmark on the Gettysburg battlefield, is on land that belonged to Nicholas' maternal grandmother's family. Elizabeth Ellen Spangler completed a teacher's training course at the Shippensburg Normal School. She sang in the church choir at Arendtsville, and it was there that she met the Reverend Samuel Nicholas. They were married in Arendtsville on June 17, 1894, and John Spangler Nicholas, their only child, was born in the Troy Hill section of Allegheny, now part of Pittsburgh, on Sunday, March 10, 1895, between 7 and 8 A.M., in time for an announcement to his father's congregation at the eight o'clock morning service. He was named John for his paternal grandfather, John Nicholas; his parents called him Spangler, but to his friends, at least from his college days onwards, he was Nick.

His parents worked indefatigably in the service of the church, and Elizabeth Nicholas, no less ardent a worker than her husband, was very popular with the parishioners. She

devoted herself selflessly to parish duties even when her son was a young child. He was therefore entrusted to the care of Sophia (Mrs. John) Diebold, whom he called Aunt Suff, and this was for him a happy arrangement, as she became almost a second mother to him and he adored her. He once said of her that she was the only person in the world who thought he was always right. To those who knew him later this offers a clue to some aspects of his complicated personality. The approval of his friends and colleagues was always important to him, and even in his laboratory he was an intensely social being.

Nicholas' father was a short man of only five feet four inches; he was one of a large family and called himself "the runt of the litter." He had had a broken leg badly set; his son said that it was in attempting to compensate for what he regarded as his handicaps that he exerted tremendous energy and tenacity in securing the goals he set for himself. Things in life came to him, according to his son, with difficulty and with real effort. The son was later to exert similar energy and tenacity, although things in life were to come easily to him.

Nicholas, the son, was physically well built and extraordinarily well coordinated, a feature that was to stand him in good stead in his chosen profession. His physical resemblance was to his mother; he had her charm and ready friendliness, and presumably he inherited from her his musical talents, of which we shall speak further below.

Both parents were truly interested in their son's intellectual welfare. His father taught him Greek; his mother tutored him at times, and from her, he said later, he learned correct habits of study. Nicholas' parents hoped that their son would enter the Lutheran ministry. But when Nicholas entered college he planned to choose medicine for his profession. This decision resulted in considerable part from the influence of his uncle, Dr. Harry Spangler, a surgeon who practiced in

Carlisle, Pennsylvania. Nicholas described later the vivid impressions made on him, when he visited Carlisle as a young child, by the experiences of accompanying his uncle on calls and particularly by the fact that his uncle had talked to him about cases as though he were an adult.

Nicholas attended primary and secondary schools in a number of communities in Pennsylvania as his father moved from parish to parish. After graduating from high school in Middletown, Pennsylvania, he matriculated at Pennsylvania (now Gettysburg) College in the autumn of 1912. In 1910 his father had taken him to attend the inauguration of W. A. Granville as president of Pennsylvania College. Granville, a Ph.B. and Ph.D. of Yale University, came to Pennsylvania from the Sheffield Scientific School at Yale, where he had been a popular instructor in mathematics. Nicholas said later that the inauguration was an event which had a great effect on the course of his life, since his decision to attend Pennsylvania College (where his uncle Harry Spangler had also graduated) was made, at least tentatively, at that time. His final decision was made partly out of loyalty to his father, but other factors entered into it too. In earlier years Pennsylvania College had been strongly denominational, but under the administration of Granville, its first president not of the cloth, it changed considerably in this regard. But in 1912, when Nicholas entered college, as now, adjustments in tuition fees were made for sons of Lutheran ministers. The Reverend Mr. Nicholas' salary in those years was hardly adequate to see a boy through four years of college.

Nicholas toyed briefly with the idea of attending the United States Military Academy at West Point, and had made some moves, through his uncle Harry Spangler, toward obtaining appointment to the class which graduated from the Academy in 1916. He took the physical examinations and visited the

Academy, but he finally decided against attending because there seemed to be nothing but peacetime army life to look forward to. He never regretted his decision in favor of Pennsylvania College, and felt later that he had made a fortunate choice and that his college had prepared him well for his professional career.

Among his college courses he had one year of mathematics (calculus), two of physics, four of chemistry (including a lecture course in industrial chemistry), and two double courses in biology taken during his junior and senior years. Chemistry and physics were taught by Johns Hopkins doctors of philosophy, one a former student of Remsen's, the other a former student of Rowland's. Dr. George Diehl Stahley, an M.D. of the University of Pennsylvania, who preferred teaching to practice, taught biology, and he permitted students to enter his courses only after they had studied at least one year of physics and two of chemistry—a practice only now, fifty years later, beginning to become generally accepted in American colleges and universities. Biology caught the young Nicholas' fancy as no other subject had, and it was thus under the influence of Stahley, "Bones" Stahley to his students, that Nicholas modified his original aims and shifted his interest from medicine to biology.

He was introduced early to independent work in biology, both in research and in teaching. As a junior in college he carried out, as extra work in an advanced physics course, an investigation on the physiological effects of light of controlled wave-length. Also, during his junior year, Stahley suggested to him that during the summer of 1915 he might study botany at the Cold Spring Harbor Laboratory under John W. Harshberger of the University of Pennsylvania, and bacteriology at George Washington University under Oscar Hunter, with a view to teaching these subjects at Pennsylvania College in the fol-

lowing academic year. This plan was carried out, with the result that during his senior year Nicholas was taking six courses (one the second year of the double course in biology) and teaching two, and he was listed in the 1915-1916 catalogue as an Assistant in College Biology. He received his B.S. degree at Pennsylvania College in 1916; the College awarded his father the honorary degree of Doctor of Divinity the same year.

Harshberger had suggested that Nicholas come to work with him at the University of Pennsylvania after completing his college work, but Nicholas chose instead to remain at Pennsylvania College for an additional year, taking an M.S. degree in 1917. During this year he continued to teach bacteriology and botany at Pennsylvania, holding the same title as during the previous year. He also taught beginning Latin and Greek and upper-term English at Gettysburg Academy, where for the first time he was officially called Master—a designation that was to mean much to him in another connection later.

Nicholas chose to go to Yale the following year. This was probably the most important decision of his life, after he once had decided to become a biologist, and President Granville was influential in helping him to arrive at it.

Nicholas, as a college student, had grown to know President Granville, and also his family, Mrs. Granville and their two daughters, very well. Nicholas recognized early that Granville was a thoughtful scholar, and developed great admiration for him. As a freshman he had joined a fraternity (Phi Kappa Psi) one of whose members and officers married the President's older daughter. This brought Nicholas easy entrée into the President's home, and he became a close friend of Mrs. Granville and of the Granville family. He said later that the Granvilles played an increasingly important part in his life throughout his college years. His closest acquaintance with them came during his junior year, when one of his fraternity brothers

developed a lung abscess as a result of a football injury and was taken into the Granvilles' home, where the fraternity members acted as nurses for three weeks until he died. Living in the Granvilles' house under these conditions brought Nicholas even closer to the family than he had been before, and his bonds of affection to the President thus were tightened. Tight bonds of affection were always important to Nicholas, as we have intimated before.

Against this background it is easy to see why Nicholas took seriously Granville's advice to go to Yale. But the move from Gettysburg to New Haven was not an easy one to make in the autumn of 1917 when the winds of war were blowing strong. While at college Nicholas had spent a summer at the voluntary Officer's Training School founded by Leonard Wood at Plattsburg, New York. When the United States entered World War I in April 1917 he telegraphed the Adjutant General's Office in an attempt to volunteer, expecting an automatic commission as a result of his experience at Plattsburg. When he did not receive an immediate reply, he finished his year of teaching, on Granville's advice, and then spent the summer as a counselor at a boys' camp in New Hampshire. While in New Hampshire, he received a telegram from the Adjutant General's Office asking him to report for physical examination at Fort Oglethorpe. The examination was scheduled for a date two weeks before he received the telegram, according to Nicholas, and his request to take the examination at Plattsburg was refused. Granville advised him, under the circumstances, to go to Yale until called up for duty, and this he did. At least, he went to Yale; he soon called himself to duty.

He entered Yale as a graduate student and as a graduate assistant in the Department of Zoology in the fall of 1917, thus beginning an association with Ross Harrison that was to be of dominant importance to him for the rest of his life. But

since Nicholas did not complete the work of that academic year, discussion of his activities as a graduate student may be deferred until after his war activities have been described.

Events had a way of turning themselves to Nicholas' advantage (which says something important, of course, about Nicholas himself), and he felt later that the failure of his first attempt to volunteer had been providential. Not willing to wait to be drafted, he enlisted in the Army Medical Corps as a private on March 5, 1918; it was possible in those days to volunteer for a particular service in the Medical Corps. He was sent to Fort Meade for basic training and was then assigned to the Vaccine Department of the Army Medical School in Washington, D.C. Here he could capitalize on his earlier bacteriological training, and he was one of several who worked to improve methods for increasing production of typhoid vaccine. Nicholas found that a type of agar used abroad as a growth medium, but with unpredictable results, consistently produced high yields if maintained at a constant pH. But when grown under these improved conditions the bacteria were sometimes atypically long, even stretching, as he described them, across the whole microscope field. He worked out a modification of the Barber device to isolate the atypical forms, developed mass cultures from the isolates, injected them into cats, which developed symptoms of typhoid, and then isolated from the cats typhoid bacilli normal in form. I do not know whether these discoveries were turned to the advantage of the American Expeditionary Forces, but they serve to illustrate the scientific ingenuity that already characterized Nicholas' mind.

Nicholas was discharged from the Army on January 9, 1919. He was later, from 1924 to 1936, to hold a commission as First Lieutenant in the Sanitary Officers Reserve Corps. On his discharge from the Army in 1919, however, he returned to the Osborn Zoological Laboratory at Yale. As a graduate student,

as a graduate assistant in his earlier years, and as Currier Fellow in his final year of graduate study, he was a hard worker, and already at that time he became very devoted to Harrison, concentrating anxiously on measuring up to the high standards of workmanship set by Harrison for all members of the laboratory. While he was a graduate student, the laboratory was literally his home; he lived in quarters for graduate students established in one of its towers. The laboratory represented a figurative home to him for most of his subsequent life; only once was he officially to leave it again for any appreciable number of years, and even then he worked there during at least some of the summers.

Nicholas received his Ph.D. in 1921; he went to the University of Pittsburgh in the autumn of 1921 as Instructor in its Department of Anatomy. Shortly after, on December 17, 1921, he married Helen Benton Brown of New Haven, who survives him. He spent five years at Pittsburgh, one as Instructor, then four as Assistant Professor. The work of these years had a very special character of its own which proved important to his further development as a biologist.

Nicholas was invited to come to Pittsburgh by Davenport Hooker, who had been the first student to earn a Ph.D. with Harrison at Yale. Hooker also had a number of connections with Nicholas, other than through Harrison, that may have brought Nicholas' qualities to his attention. In 1917 Hooker had married Helen Ferris, the daughter of Harry Burr Ferris, who taught gross anatomy at Yale; for many of Harrison's years at Yale it was a requirement of the Department of Zoology that minors be elected outside the Department, and a number of the students, even as late as the 1930s, elected to take part of their minor work in human anatomy at the Medical School. Nicholas did so; the course under Ferris' direction was a particularly lively intellectual experience—Hooker said of Ferris in a memorial note that, when he taught, bare bones

seemed to take on flesh before the students' eyes—and was highly stimulating to Nicholas. Hooker himself spent his summers in Clinton, Connecticut, and may well have seen Nicholas in the dissecting room at Yale. Furthermore, Mrs. Hooker's uncle Richard Shelton Kirby had taught at Gettysburg during Nicholas' first three years there and had known him. In 1915 he moved to New Haven where he lived when Nicholas was a graduate student; his house was another meeting-ground for Hooker and Nicholas when the latter was a graduate student.

Hooker was head of the Department at Pittsburgh when Nicholas came there; John Donaldson, the son of H. H. Donaldson so distinguished for his work on the nervous system of the rat, arrived the same year as Nicholas. The working conditions at Pittsburgh were almost unbelievably primitive at the time; according to Donaldson, the laboratory in which Nicholas worked would not now be acceptable as a janitor's closet. Its sole plumbing, a single sink, was installed personally and conjointly by Hooker, Donaldson, and Nicholas, who also cooperated in such activities as inventing bookstands for the dissecting room based on movable No Parking signs, which still remain in the Department, and establishing a flourishing rat colony, as a natural result, John Donaldson said, of family background. The rats lived in a penthouse on the roof. Next to them, in a little hut, were kept cats that Nicholas was using for endocrinological experiments to be described below; the cats learned to find unsealed areas in the baseboards and vanished into the very thick hollow tile walls of the building to be retrieved four floors below, hungry but undamaged, when their yowling indicated their positions. Yet Nicholas enjoyed some of his most productive years in these surroundings; small wonder that in later years his colleagues' expressions of dissatisfaction with more modern facilities did not distress him to the degree that the complainers might have wished.

Hooker, Donaldson, and Nicholas were a remarkable team;

they were co-equal not only as plumbers and animal caretakers but also as instructors. Each of the three taught one section of the first-year anatomy course, and each taught his section as he thought best, with one of the other two as assistant. There was no limitation except that a certain portion of the work had to be covered in the allotted time; whoever assisted would loyally carry out the ideas of the principal teacher of the moment. No teaching experience could have been better designed to stimulate the imagination of one as independent and eager as the young Nicholas.

The course was also unique in that it seems to have been the first in the country to combine gross, microscopic, and developmental anatomy into a single unit. This too had its historical backgrounds: as a result of Harrison's German training, the influence on him of Gegenbaur's concepts, and his own large view of things, his students learned vertebrate morphology as an integrated discipline interrupted by no artificial barriers between embryo and adult or between cell, organ, and organism. Donaldson had studied pathology with Welch, who had dealt with all its phases, gross and microscopic, as a unified subject. Working to integrate many seemingly disparate facts and ideas would have been particularly satisfying for Nicholas, whose life always encompassed multifold activities simultaneously.

Although the five years at Pittsburgh were the only ones that Nicholas spent within the confines of medical school walls, he remained close to medical thought throughout his life. He was a wise adviser at Yale to countless premedical students; for many years he was an examiner for the Connecticut State Board of Healing Arts. His whole philosophy of medical education, in which he always maintained a keen interest, was based on his Pittsburgh experience.

The Pittsburgh years were also particularly favorable for the development of Nicholas' research. Nicholas taught not

only the combined anatomy course to first-year students but also, for four years, neuroanatomy to second-year students, which reinforced his already growing interest in the nervous system. His needs for uninterrupted time for his own work during the *Amblystoma* breeding season were recognized and honored. Equally important, the rat colony that had been set up through the cooperative efforts of Hooker, Donaldson, and Nicholas provided abundant material when Nicholas began to perform his experiments on rat embryos.

In spite of the success of his work at Pittsburgh, Nicholas nonetheless returned in 1926 to the Department of Zoology at Yale as Assistant Professor, and he remained in the Department for the rest of his life. He was promoted to Associate Professor of Comparative Anatomy on the Bronson Foundation in 1932, and was appointed in 1935 to the Bronson Professorship of Comparative Anatomy. This was the chair in which Harrison had been the first incumbent when he had come to Yale from The Johns Hopkins University in 1907. Harrison had vacated this chair to become Sterling Professor of Biology in 1927. Nicholas had fewer years to wait for the appointment to a Sterling Professorship in Biology. He received it in 1939, and held it until he became Emeritus in 1963. He was Chairman of the Department of Zoology from 1946 to 1956, and Master of Trumbull College from 1945 until his retirement.

He devoted much of his time and many of his energies (and they were plural) throughout the years to duties other than those directly related to his own teaching and research, but from the time he began his research activities as a graduate student at Yale he felt his research to be by far the most important of his varied interests. Accordingly, it is appropriate to devote the central section of this memoir to a consideration of the varied subjects of his investigations.

Embryology was the primary one, and in fact he chose to

study at Yale, under Granville's advice, as the result of an express desire to work with Ross Harrison in this field. Harrison had, by the time Nicholas began his embryological studies, performed important pioneering experiments on the development of asymmetry in the vertebrate limb. Using embryos of the spotted salamander, *Amblystoma punctatum*, as it was then officially known, he had shown that whether a forelimb rudiment would form a left or a right limb is determined in some respects by the relationship of the limb rudiment to its surroundings in the embryo. At the stage at which Harrison worked, a rudiment of the forelimb develops into a limb corresponding to its side of origin if its dorsoventral axis is placed in normal orientation with respect to the host, and into a reverse limb if this axis is inverted at transplantation. These rules hold whether the grafted limb is placed on its side of origin or on the opposite side of the body. Harrison had found that in some cases, even though the transplanted limb rudiments followed the above generalization, the limbs regained their normal posture later by adjusting their position. In the work for his dissertation Nicholas attempted to analyze the factors responsible for such regulation of posture. His experiments altered the location of the grafts, their size, their age at transplantation, and the degrees of rotation through which they were moved before transplantation. These experiments were thus simple variations of those already performed by Harrison, who had rotated his grafts through 180° . Nicholas rotated the disc representing the limb rudiment through either 90° or 270° before transplantation. He found that the limbs resulting from 90° rotation recovered their typical posture by rotating counterclockwise, and those resulting from 270° rotation rotated clockwise (thus in each category through the shorter arc). He showed that the factors instrumental in effecting the rotation related primarily to the development of the skeletal limb girdle.

The definitive results of Harrison's experiments were presented in a highly influential publication that appeared in 1921. Nicholas, as we have said, was awarded his doctor's degree in 1921, but the paper based on his dissertation did not appear in print until 1924. It has been stated in another memoir of Nicholas that the delay resulted from Harrison's perfectionism and that the thesis was repeatedly rewritten before its publication. This explanation is not complete. It is quite true that Nicholas' writing has on occasion come under some criticism. His mind worked with great speed, and as a result he sometimes expressed himself, both orally and in writing, elliptically and somewhat unconventionally, but his ideas were expressed with great vigor, and readers of his papers cannot escape feeling the force of his intellectual power. With respect to the delay in publication of the dissertation, comparison of the original dissertation with the published version shows that, although the latter is considerably shorter than the original, there were few drastic literary changes; the real difference between the two versions resides in the content. Nicholas had performed extensive additional experiments, differing from his original ones in the number of degrees through which the grafts were rotated, before completing the paper for publication.

Both in the dissertation and in the 1924 publication based upon it, Nicholas stated that experiments had been performed in which the immediate surroundings of the limb were rotated, but that these experiments were still incomplete and would be described later. The experiments, which involved a double operation, were ingenious; preliminary results were reported at the December 1921 meetings of the American Association of Anatomists, and the abstract states that, when the limb bud is oriented normally with reference to the embryo but the surrounding tissue rotated, the limb develops in such a way that it assumes its posture with reference to

the surrounding area and not to the organism as a whole. A progress report on this work was delivered to the American Society of Zoologists at its December 1924 meetings, and some further details relating to the development of the limb girdles after such experiments were presented at the April 1926 meetings of the Anatomists. The results and the experiments were never described fully in print, although some of them were incorporated in a chapter of a compendium volume on experimental embryology published in 1955. Nicholas' final publication describing the effects of rotating the surrounding tissues on the asymmetry and posture of the limbs did not appear until 1958, more than thirty years after the experiments were performed. The results, however, were known to investigators of limb development and were frequently referred to earlier in the literature. In fact, the experiments were carried further by F. H. Swett who reported in 1938 that the effective portion of the surrounding tissue is that originally dorsal to the limb rudiment; he explained the results in terms of a barrier set up here against the actions of factors resident in various other parts of the embryonic body. The facts that Nicholas himself had not carried this experimental analysis to an end, and that he did not publish until 1958 the results of experiments begun in 1921, are easily accounted for. He was either completing or beginning too many new experiments of other kinds to be able to devote further attention to the earlier ones.

As a matter of fact, some of the varied paths into which he diverted his energies led from the same points of origin as the work for his dissertation, and he began to follow some of them out before the work of his dissertation was very far under way. Even as a graduate student he did not choose to concentrate his efforts on a single problem. During the academic year 1918-1919, thus presumably immediately after returning to Yale from the Army, he made, at Harrison's sug-

gestion, limb bud grafts to the dorsal or ventral midline. It would seem that these must have been among Nicholas' first operations; in the memoir of S. R. Detwiler that he prepared for the National Academy of Sciences he wrote that Detwiler gave him his first lessons in surgery on *Amblystoma* in the spring of 1919. Theoretically, it would have been expected that the midline grafts, if their anterior-posterior axes conformed to those of their hosts, would form double limbs, each component of the pair with its asymmetry corresponding to the side upon which it develops. If the anterior-posterior axis of the bud is reversed, the resultant limbs should show reversed asymmetry. Nicholas reported the successful performance of such experiments, with a few grafts showing the predicted results, at the December 1921 meetings of the Anatomists; the definitive publication appeared in 1924, five months before the article based on his dissertation.

Meanwhile Harrison and Detwiler had been studying for some time the relationships of the nervous system to transplanted limb buds; in fact, Harrison's motive in his first amphibian limb bud transplantations (1907) had been to investigate their bearing on the development of the nervous system. Nicholas, in his 1924 paper on limb bud grafts to the dorsal midline, made some preliminary observations on the effect of these grafts on the underlying spinal cord. As a result of these observations he performed further experiments in which grafted limb buds were utilized as barriers isolating portions of the central nervous system. Detwiler had studied in other ways the effects of the medulla on the differentiation of the spinal cord, and Nicholas saw that the interposition of the limb as a barrier between different parts of the nervous system could further elucidate such effects. A long article by Nicholas published in 1929 in the Festschrift in honor of Hans Spemann's sixtieth birthday reported an analysis of the re-

sponses of parts of the *Amblystoma* nervous system isolated in this way. This paper reported the use of limb grafts to separate the medulla from the spinal cord or to fill gaps caused by the removal of the segments of the cord which innervate the limbs. In experiments forming the basis of another paper, published the following year (1930), the mesencephalon was removed and limb bud grafts were used to block the later junction of diencephalon and medulla. These experiments were not followed by quantitative changes in the first five segments of the cord, and thus confirmed Detwiler's earlier demonstration that the medulla is the effective portion of the central nervous system influencing the proliferation of cells within the cord. In 1931 Nicholas described in short notes the effects of transplanting an extra medulla; these, again confirmatory of earlier results by Detwiler, seem not to have been described *in extenso*. Nicholas' last report on transplantations within the amphibian central nervous system appeared in 1956; this was a short paper in the *Proceedings of the National Academy of Sciences* describing the effects of reversing strips of neural plate together with underlying chordamesoderm; the degree of regulation which followed varied according to which of the axes were reversed.

There was another set of sequels, in Nicholas' work, to the experiments that he began in 1919 by transplanting limb bud grafts to the dorsal midline. In his preliminary report of these experiments (1922), and in the 1924 paper describing them more fully, he recorded the observation that these limbs were innervated by nerves which do not normally supply them. Nicholas first reported in 1928 innervation of the dorsal limb grafts by cranial nerves, and in 1929 he published a preliminary note dealing with movements in ectopic limbs (including some developing in the orbit of the eye) innervated by cranial nerves. A long paper in 1933 dealt with the correlation of nerve sup-

ply and the movements of the transplanted limbs. The last paper Nicholas published on limb movements was written in collaboration with D. H. Barron, and reported the results of electrical stimulation of dorsal and ventral nerve roots of normal and experimental axolotls of about six months of age. These results emphasized the importance of sensory components in coordinating the activity of groups of muscles.

The significance of the studies on developing embryos described above, closely related to studies on these subjects by Harrison, Detwiler, and Swett, was that they contributed to the growing body of knowledge on limb asymmetry, posture, innervation, and movement. Nicholas, however, also carried out numerous other analyses of amphibian life and development. His very first publication, an abstract of a paper delivered to the Zoologists in December 1920, was a note verifying the fact that *Amblystoma tigrinum* reacts to olfactory stimuli; the experiments on which this was based, carried out at the instigation of Henry Laurens, were begun, like the experiments on limb transplantation, during the spring of 1919, and Nicholas' first definitive paper, published in 1922, described these experiments and their results. Then he recorded in 1924 and 1925 observations on the presence of a balancer in *Amblystoma tigrinum*, a species previously described by others as lacking this organ. These were modest studies, but they serve to show that the diversification of Nicholas' biological interests began very early during his scientific career.

Although he later moved farther afield among the vertebrates, Nicholas never abandoned his interest in the development of *Amblystoma*. In 1945 and 1948 he reported the results of studies, carried out by vital staining, on endodermal movements in this form during pregastrular stages; these were important and original in that they pointed up the need for investigation of morphogenetic movements during blastula

stages, hitherto largely neglected. In 1952, when embryology was entering its present biochemical phase, he collaborated with Edith Krugelis in an investigation of the effects of temperature on alkaline phosphatase activity and nucleic acids in *Amblystoma*; variations in temperature were shown not to produce alterations in the phosphatase activity nor in DNA production, but lowering the temperature increased the production of RNA under the conditions of the study. In 1963 he published a paper describing movements of the surface pigment of cleaving *Amblystoma* eggs as studied by time-lapse cinephotomicrography; this was his last full paper to appear. Four years before, he had published an earlier paper on this subject reporting investigations begun in 1926 or 1927 and continued intermittently ever since, and in the 1959 paper he stated that his original interest in this problem had been stimulated in the 1920s by comments of Harrison on normal development.

Thus his work on amphibian development began and ended in ideas emanating from the work of Harrison. This was not always to be the case for his investigations in other than amphibian embryology, and to consideration of these we may now proceed. It must be evident from the foregoing paragraphs that Nicholas' fashion of work was not such as to permit the construction of an orderly chronological record of his investigations. Accordingly these may be (as they are here) arbitrarily classified on the basis of the vertebrate groups he studied and the problems raised by their development.

Let us first take up his contributions to teleost embryology. He had made his first visit to the Cold Spring Harbor Laboratory, it will be remembered, during the summer following his junior year at college. In 1925 he returned to the Laboratory for the summer as an assistant in Comparative Anatomy; the following summer he assisted in courses in Com-

parative Anatomy and Experimental Surgery, and in the summer of 1927 he headed the course in Surgical Methods in Experimental Biology (and in this year he became a member of the Scientific Advisory Committee of the Laboratory, a membership which he maintained through 1940). By 1927, his fourth summer at the Laboratory, he was unable to resist the challenge offered by the marine embryos available there, and accordingly he turned his attention to the study of the development of *Fundulus heteroclitus*.

The eggs of this common minnow, the killifish, had previously been subjected to study only by indirect methods, since its shell, the so-called chorion, had previously seemed impossible to remove without injury to the egg. By devising a clever method, still used by a number of investigators of fish development, of dechorionating the egg with especially modified eye-surgeons' iridectomy scissors, he made it accessible to direct microsurgical manipulation; thus he began the modern studies on fish eggs in America. A description of the method he used for removing the shell was published in the autumn of 1927; again, conforming to a familiar pattern, final publication of the results of the experiments was deferred until 1942, when they were presented in an article published jointly with the author of this memoir, one of his former students. All the experiments on premitotic stages described in this paper were performed by Nicholas. These were defect experiments, but they were far more refined than defect experiments previously performed on *Fundulus* eggs by others, who had merely poked needles through the intact chorion. Nicholas' results demonstrated the powers of regulation inherent within the developing teleost egg. Investigations of teleost development, described in publications which appeared almost simultaneously in the 1930s in Europe and in the United States, were among the first to broaden Spemann's concepts of embryonic induction

to include other than amphibian development. Had Nicholas' 1927 results been fully published earlier, he would have received the credit he deserved for being the first to apply modern methods of experimental embryology to the study of teleost development.

He also applied such methods to the study of mammalian development. His experience with mammalian experimentation dated back to the time of his early years at Pittsburgh. During the summer of 1923, when he was visiting Mrs. Nicholas' family in New Haven, he often came to the Osborn Laboratory to read journals. Dr. W. W. Swingle, then Assistant Professor of Biology at Yale, asked him if he would like to give him a hand removing parathyroid glands from cats. Nicholas accepted the invitation with alacrity. He was so skillful at the surgery that Swingle invited him to collaborate with him on a detailed study of parathyroid tetany in cats.

Swingle and Nicholas began by extirpating the glands in the cat. Before 1925, when Collip and his collaborators first reported the endocrine function of these glands, the literature contained conflicting reports concerning their dispensability in the cat. Swingle and Nicholas demonstrated that complete parathyroidectomy is invariably fatal in the cat, and that the survival of some cats after apparently complete parathyroidectomy is accounted for by the presence of accessory glands that have not been removed. They reported, also, the results of auto- and homo-transplantations of the gland, demonstrating the successful survival and function of the autoplasmic grafts.

Preliminary reports on these experiments appeared at the end of the year 1923. According to Swingle, Nicholas contributed generously of his time and energy in this collaborative work, which continued through several years, and Swingle thinks that the work might never have been completed with-

out Nicholas' suggestions, operative skill, and unflagging interest. That Nicholas made this substantial contribution to endocrinological progress was probably remembered by only a few of his friends during his later years, and may come as a surprise to some of his younger colleagues, although a number of students participated in his graduate course in endocrinology during the years he taught it at Yale.

The last of the joint publications with Swingle on the parathyroids appeared in 1925, but Nicholas' knowledge of endocrinology was useful in various ways in his later work on mammalian development. Methodologically it was of advantage to him. In his first paper on mammalian development, also published in 1925, he referred specifically to a surgical procedure used by Biedl, to whose work Nicholas had referred in the papers on the parathyroids. Also, for the handling of mammalian embryos *in vivo* and *in vitro*, an understanding of the role of endocrine factors in reproduction is essential, on theoretical as well as practical grounds. Nicholas was later to deliver several talks at scientific meetings on various aspects of reproductive physiology in the rat, and during the 1940s he published two papers reporting the results of work in this area. One of them was a joint publication with J. A. Carmosino, who died before the paper appeared in print. Carmosino had been for over fifteen years the caretaker of Nicholas' animal colony at Yale; Nicholas' manner of working with him was such that he had not only been a devoted assistant but had become an active participant in the work itself.

Nicholas' 1925 paper on the application of experimental methods to mammalian embryos states explicitly that the developmental investigations were begun at Pittsburgh in 1923, and it gives this reason for starting them: "Recent studies upon the transplantation of eyes in adult mammals by Kópányi ('23) have rendered it imperative that more should be

known about the early mechanics of development in the mammal." One of Nicholas' colleagues at Pittsburgh remembers how skeptical Nicholas was of Koppányi's results at the time they were published; thus there is little reason to doubt that their publication was in fact the event which stimulated Nicholas' initiation of his own studies on mammalian development.

His investigations on mammalian eggs, embryos, and fetuses were carried out almost exclusively with the rat. When the 1925 methodological paper was sent to press, Nicholas knew that E. A. Swenson had published a paper the same year concerning methods of procedure in studying mechanical and electrical stimulation of rat embryos. By the time Nicholas published his second paper on mammalian development the following year, he had seen Ernst Bors's 1925 publication which also described the application of experimental methods to the study of mammalian development, this time in the rabbit. Thus Nicholas was one of the first investigators to work in this field, and his studies were begun independently without knowledge of the fact that others were developing similar interests. Nicholas was the only one of the three initial workers to carry out a substantial program, and he continued it actively throughout the rest of his life. Of all his varied embryological studies, his experimental analysis of the development of the rat was probably of centralmost interest to him. Performing the delicate operations on rat embryos was probably the greatest pleasure in Nicholas' life, and he enjoyed carrying out the experiments more than writing about them. Furthermore, the experiments were diverse; various types of experiments were carried out simultaneously; and new kinds of experiments were often begun before older ones were written up. Thus again, as in the case of the amphibian studies, they are difficult to discuss on a chronological or any other systematic basis.

It seems clear from the record, however, that Nicholas' first experiments on mammalian material were extirpations of the limb, the eye, or the tail at relatively late fetal stages, that is, between one and nine days before birth. The first paper (1925) discussing the results of these experiments demonstrated that, during the final third of gestation, amputation of the limb is not followed by regeneration; this was the experiment Bors had reported for the rabbit with the same result. The result, of course, simply suggested to Nicholas that he must study earlier stages of development, which he soon was to do, but meantime there was still at least one major problem to be investigated by the methods he had developed for experiments on the older fetal stages. This was to ascertain the results of spinal cord section in fetuses, and this investigation was carried out jointly with Davenport Hooker, who was senior investigator during this collaboration. The operative procedures and the testing of the living fetuses were performed by Nicholas; Hooker examined the sections and interpreted the histological findings.

The first reports on these experiments, and their interpretations, were made in 1927; Nicholas had by then developed his own interest in the nervous system, and since 1915 Hooker, with positive results, had been studying in frog embryos the regeneration of the spinal cord transected early in development (closed neural-fold stage). The cord transections on the rats were made on embryos and fetuses at between twelve to eighteen days of gestation, either by cutting or by electrocautery, and the results were negative with respect to regeneration. Some previous authors (including Gerard and Koppányi in 1926) had tentatively claimed, on the basis of physiological tests alone in the absence of histological study, that cord regeneration might occur after transection in mammalian fetuses. Hooker and Nicholas were able to explain away the physiological results which led to suspicion of regeneration on the basis of transmission of impulses along collateral paths. Although abortive regeneration

has subsequently been demonstrated in the spinal cord of rats, a man or other mammal whose cord is traumatically severed may still expect to remain paralyzed below the site of the injury for his lifetime.

Before the study with Hooker was completed, Nicholas had begun other types of experiments on rat fetuses. In 1929 he published a short communication describing attempts to transplant the eye from one fetus to another. Two years later (1931) he reported attempts to maintain rat embryos of eight to nine days of gestation age in various conventional salt solutions *in vitro* or in various ectopic positions in the mother: he placed a horn of the uterus in a subcutaneous pocket in the body wall; he separated the upper tube from the uterus three days after fertilization, releasing segmenting eggs into the abdominal cavity; he transplanted eight- and nine-day embryos to the outside body wall, to the femoral fascia, and to various subcutaneous areas. Some degree of differentiation occurred, the best in transplants to the mammary gland, and Nicholas commented in a brief paper in 1931 that the grafts resembled the chick grafts described by Willier and Murphy after transplantation to the chick chorio-allantois.

In 1930, at the invitation of Willier, Nicholas spent one of his rare summers away from New Haven teaching comparative anatomy and experimental morphology at the University of Chicago. Here he saw chick chorio-allantoic grafts at first hand. He later encouraged Dr. Dorothea Rudnick, who had in the meantime received her degree with Willier and who was an expert at chorio-allantoic grafting, to come to Yale to work with him, and thus began a fruitful collaboration.

In 1931 a short paper and in 1933 a longer one, both published jointly by Nicholas and Rudnick, described the development of rat embryos grafted to the chick chorio-allantois. Next, Nicholas and Rudnick began to work on the development of

rat embryos *in vitro*. They described in 1934 and again in 1938 the development of embryos of egg-cylinder to head-fold stages explanted to a culture medium composed of rat plasma and rat embryo extract, and they reported that some differentiation occurred. They also described briefly in 1937 the explantation *in vitro* of transverse pieces of nine-day rat embryos, but did not feel that the degree of progressive differentiation attained in these experiments warranted pursuit of the subject. Nicholas' next-to-last publication was an abstract of a paper presented at the 1963 meetings of the Anatomists describing, jointly with Bette Duff, the development of such fragments in the anterior chamber of the eye.

Nicholas designed a special chamber that permitted the circulation of fluids for studies *in vitro* (1937) and in 1938 he reported that the use of this chamber improved the differentiation of explanted rat eggs and embryos. Also, he published a number of extensive papers in the 1930s and 1940s (and one in 1950) on the potentialities of rat embryos for development after transplantation to various abnormal sites *in vivo*. These were summarized in the 29th Mellon Lecture, delivered at the University of Pittsburgh School of Medicine in 1946; the lecture was published in revised form in the *Quarterly Review of Biology* the following year.

According to Dr. Rudnick, who collaborated in much of the work, the transplantation and explantation experiments agreed in showing that the period of early axis formation in the rat blastocyst (*ca.* nine to ten days after insemination) was the most favorable moment at which to secure progressive differentiation outside the uterine environment. The extent of progress possible was of the order of some two to five days of normal development. In the grafts, differentiation of the established germinal regions was predominantly at the tissue level. *In vitro*, morphogenetic changes,—e.g., folding, outgrowth, somite formation, etc.—

tended to parallel or even outpace tissue differentiation. All these experiments were essentially exploratory and were dropped when it became clear that only limited development was to be obtained in the various experimental environments devised at that time. Nicholas' hope had been to find an accessible situation where the mammalian embryo could be carried through, if not to term, at least to a stage of active neuromuscular function.

Nonetheless, the application of the methods of experimental embryology to mammalian eggs and embryos would seem to have been Nicholas' most important direct scientific contribution. Perhaps his most significant set of experimental results in this area remains to be specified here. One of his graduate students, B. V. Hall, in studying the changes occurring in the uterus during pregnancy, found that the *zona pellucida* of the rat's egg softens in acidified calcium-free Ringer's solution; this permitted for the first time experimental separation or fusion of blastomeres in the mammal. Nicholas and Hall (1942) demonstrated that blastomeres isolated at the two-cell stage and then transplanted into the uterus could develop to the egg-cylinder stage, and in a single case an oversized fetus seemingly produced by the fusion of two one-celled eggs was brought to term. These truly significant results, which proved the regulatory nature of mammalian development, have not received due attention in the subsequent literature. When Seidel first reported in 1952 the development of isolated rabbit blastomeres, he did not mention the work of Nicholas and Hall; in Seidel's long definitive paper (1960) the Nicholas and Hall paper is listed in the bibliography but not mentioned in the text.

During the 1940s, Nicholas' studies on the mammal, like those on the Amphibia previously reported, took a biochemical turn, and a series of excellent collaborative studies, with Heinz Herrmann, on the biochemistry of developing rat muscle was completed. Nicholas also studied, together with E. J. Boell, the

respiratory metabolism of developing rat eggs from the one-cell through the sixteen-cell stage by means of the Cartesian diver technique.

Also, as a corollary and necessity to his experimental studies, he worked out a series of normal stages for rat development. The stage series was described briefly in a chapter contributed by Nicholas to a compendium volume on *The Rat in Laboratory Investigation* (1942, 1948). He prepared photographic illustrations of the stages in 1935, and a small number of bound sets of these, dedicated to Harrison, were privately distributed, but unfortunately the detailed rat stage series, like that previously prepared by Harrison for *Amblystoma*, was never formally published.

A few words might be mentioned here as to Nicholas' technical ingenuity and skill. It may be implicit in what has already been said that his manual dexterity was exceptional, and that his imagination was fertile in enabling him to devise technical improvements and innovations, beginning at least as far back as his time as a private in the Medical Corps. Visitors to his operating room for mammalian surgery at Yale will remember that he had invented a foot-focusing device for his dissecting microscope long before such an instrument was on the market, and that he had adapted a foot-treadled Singer sewing machine as an integral part of an apparatus for artificial respiration of rats. He invented a microscope lamp, valuable for use with dissecting microscopes, that sharply focuses cool bright light within a restricted microscopic field; this is currently manufactured and marketed as the Nicholas Illuminator by the Bausch and Lomb Company, and is widely used in industrial as well as research and academic laboratories. He was also exceptionally adept at photography and in fact published a paper, in the mid-thirties, on biological photographic illustration. But his long bibliography, numbering over 135 publications, includes only one or two

entries dealing with purely technical aspects of biological teaching or research. It is to his credit, considering the pleasure with which he used his hands and his tools, that in years during which addiction to gadgetry was becoming an increasingly prevalent laboratory disease, he was never seduced by the temptations of instrumentation for its own sake. His primary interest was in experiments.

The subjects of the dissertations of the graduate students who worked with him for their degrees—over twenty, including several whose work was directed jointly with other members of the department—reflected Nicholas' own interests, embryological, endocrinological, and neurological, and accordingly were as varied as were Nicholas' own investigations. He encouraged his students to think and work independently, sometimes, at first, to their exasperation, and many of them developed new and important fields of investigation after receiving their degrees; a number of them occupy key positions in American biology and medicine.

Nicholas' teaching activities were confined principally to Yale. When the Rockefeller Institute became a graduate school, he did hold one of the early appointments there: he was a Lecturer in the Institute for the academic years 1955-1957 and Visiting Professor during the academic years 1957-1958, 1958-1959, and 1960-1961. But though he limited his teaching to a few institutions, his influence spread wide through a variety of other activities that he carried on over and above his strictly academic duties. These were so diverse that they too are difficult to discuss chronologically.

At Yale itself, Nicholas was not only a member of the multitudes of committees upon which faculty members are so often called to serve, but he also held a number of important administrative positions. He was a member of the Board of Trustees of the Sheffield Scientific School from 1940 until 1963, and

Secretary of its Board from 1941 to 1956. Of equal importance, on a different level, was his mastership of Trumbull College. In 1933, when the colleges were established at Yale, he was elected one of the original Fellows of Trumbull; in 1945 he became its Master. He continued in this position until he retired. He enjoyed intensely his administrative duties in the College, and especially his advisory relationships with the students. Together with Mrs. Nicholas he provided warm hospitality for the members of the College. While Master of Trumbull, Nicholas established close relationships between Trumbull College and St. Catherine's College, Cambridge University; he became, in 1955, an Honorary Fellow of St. Catherine's.

But, to him, the most important administrative position he held at Yale was the chairmanship of the Department of Zoology. When Harrison retired in 1938, Lorande Loss Woodruff succeeded him as Chairman. Nicholas assumed the chairmanship in 1946 when Woodruff retired from it.

As already remarked, Nicholas became deeply devoted to Harrison early in his career as a graduate student. His admiration and affection for him grew continuously throughout the years; as a result, appointment to the chairmanship of the department that Harrison raised to such high eminence marked to Nicholas the acme of his career. He retained the chairmanship until 1956.

Those of his outside commitments that were most closely related to his academic life were editorial in nature; he held a number of important editorships. He served a stint on the Editorial Board of the *Journal of Morphology* (1933-1936); he was an advisory editor for one of the sections of *Excerpta Medica* (Anatomy, Anthropology, Embryology, and Histology) from 1947 until his death, and consulting editor for the *Yale Journal of Biology and Medicine* (1949-1954); he was also President of the Board of Managers of the last-named from 1952 until 1962.

From 1938 to 1963 he was Editor of the section on Vertebrate Embryology of *Biological Abstracts*, and from 1940 until his death he was a member of the Administrative Board of the *American Anatomical Memoirs* published by the Wistar Institute of Anatomy and Biology. But to him the most important of his editorial functions were those he performed for the *Journal of Experimental Zoology*. The *Journal* was established in 1904, and Harrison was its first Managing Editor, a position which he retained until 1947. Although it was only in 1946 that Nicholas became a member of the Editorial Board of the *Journal*, Harrison had previously often consulted him about manuscripts submitted to the *Journal*. Accordingly, Nicholas had had substantial experience in reviewing manuscripts when in 1947 he succeeded Harrison as Managing Editor. Harrison continued as a member of the Editorial Board, and Nicholas in turn sought his advice about many manuscripts. Nicholas retained the post of Managing Editor until he died.

When the *Journal* was founded, experimental zoology was new and seemed a unified science. By the mid-forties, there was little new or unified about experimentation on animals, and new journals were rapidly being set up to cover areas formerly the subject of publications in the *Journal of Experimental Zoology*. Nicholas' ambition as Managing Editor was to emulate the standards set by Harrison. It was no mean feat, when zoology was losing its identity as a single unified science, to maintain the *Journal*, as Nicholas did, as one of the leading biological journals of the world.

Nicholas belonged to a number of fraternities and honorary societies: Phi Beta Kappa, Sigma Xi (National Executive Committeeman, 1954-1959; President, Yale Chapter, 1930-1931), Phi Kappa Psi, Gamma Alpha, Beta Beta Beta, Phi Beta Pi; he was a member also of the Torch Society and of Berzelius at Yale. He was an active member of many professional societies, local,

national, and international: the American Physiological Society, the American Society of Zoologists, the American Association of Anatomists (Executive Committee member, 1947-1951), the Society for Experimental Biology and Medicine, the American Cancer Society, the American Association for the Advancement of Science (Fellow), the American Society of Naturalists, the Biological Photographic Association, the Society for the Study of Development and Growth, the Connecticut Academy of Arts and Sciences, the Beaumont Club at Yale University (President, 1951), the New York Academy of Sciences, the New York Zoological Society (Fellow, and, from 1944 on, a member of the Society's Scientific Advisory Council), the International Institute of Embryology (Fellow, and, from 1961 until his death, a member of the organizing committee for its 1964 International Conference), and the International Society for Cell Biology (Secretary, VII International Congress of Cell Biology; member of Executive Committee, 1951-1957; assistant treasurer, 1951-1963). He was a member of the Advisory Board of the Wistar Institute from 1939 to 1952 when the Board was dissolved, and for the years 1956 through 1958 a member of the Biology Visiting Committee of the Brookhaven National Laboratories. His connections with the Cold Spring Harbor Laboratory have already been specified; he was a life member of the Bermuda Biological Station. He was elected to the American Philosophical Society in 1946 (Councillor, 1954-1957), and to the American Academy of Arts and Sciences and to the National Academy of Sciences in 1949. From 1955 to 1958 he was Chairman of the National Academy's Section of Zoology and Anatomy.

Nicholas' activities on the Washington scene were far from limited to membership in the Academy and serving a term as chairman of an Academy section. He played a large number of administrative roles under the ægis of the Academy and the National Research Council, and others farther afield.

In 1942 he was appointed by the National Research Council Executive Board as chairman of a committee on zoology of the Division of Biology and Agriculture of the National Research Council, but the Council has no record that the committee ever met. From 1948 to 1954 he was a member-at-large of the Division of Biology and Agriculture; he was Chairman of the Division from February through June 1948; in 1948 he became Vice Chairman. His term in the last office ended in 1952. He was also a member of the Executive Committee of the Division from February 1948 through June 1952.

He was a member at various times of a number of National Academy of Sciences–National Research Council committees. He served from April 1947, when it was first established as the Committee on UNESCO, through December 1959, on the National Research Council Committee on Science in UNESCO. From 1949 to 1952 he was a member of the U.S. National Committee of the International Union of Biological Sciences, and from 1959 until he died he was a member of the Evaluating Committee of the National Academy of Sciences–National Research Council for the National Aeronautics and Space Administration Postdoctoral Research Associateships.

In addition to participating in these activities related to the National Academy and the National Research Council, he was also active in the National Science Foundation in its early days. The Foundation was established in 1950; it appointed its first divisional committee members in its fiscal year 1952 (which began in July 1951) and Nicholas was listed as a member of the Divisional Committee for Biological Sciences for the fiscal years 1952 and 1953 (that is, from 1951 to 1953 calendar years).

This already formidable list of Washington duties has so far omitted his participation in the two fields of extracurricular administration in which he was most interested, and which deserve to be discussed in greater detail; these activities, related

to biological organizations and to personnel, began with duties assumed at the request of the National Research Council, but expanded to exert an influence well beyond the limits of their origin.

The first of these interests, which was to prove of particular significance to biologists, led to the establishment of the American Institute of Biological Sciences. A group of nine biologists, of whom Nicholas was one, wrote in August 1946 to Robert F. Griggs, then Chairman of the National Research Council Division of Biology and Agriculture, requesting that the Division take action in developing such an institute. In November 1946 a Committee on the Proposed Institute of American Biologists was appointed by the National Research Council to assist in organizing the Institute, and Nicholas was a member of this committee. His views were of particular value to it as a result of his having been President of the American Biological Society during the previous year (1945). This society had, according to Nicholas, originally been set up to aid *Biological Abstracts*, but had in time expanded its sphere of interest sufficiently to draw up, together with the Union of American Biological Societies, of which Robert Chambers was then President, a program for an organization that would aid biologists and biology more generally. This program had been published, under the authorship of Nicholas and Chambers, in the *American Naturalist* in February 1946. The Committee on the Proposed Institute met twice, once in 1946 and once in 1947; at the second meeting it called itself the Advisory Committee on the American Institute of Biological Sciences. It recommended that the National Research Council establish the Institute within its Division of Biology and Agriculture, and that an organizing board for the proposed Institute be constituted; Nicholas became a member of that board. The American Institute of Biological Sciences was formally established in February 1948. As Chairman of the Division

of Biology and Agriculture, and later as Vice Chairman, Nicholas was *ex officio* a member of the Executive Committee of the Governing Board of the Institute. When his term as Vice Chairman of the Division ended, he continued to serve on the Executive Committee of the Institute through 1955. The Institute became independent of the National Research Council in 1955, but Nicholas remained a member of its Governing Board until 1959. He also served on various AIBS committees: from 1948 until 1951 on its Committee on Selective Service; from 1949 through 1951 as Chairman of its Committee on Advisory Service to the Armed Forces; from 1951 to 1952 as Chairman of a Committee on the AIBS Roster of Biologists; from 1952 to 1954 as Chairman of the Subcommittee on Procedures of the AIBS Committee on the Handbook of Biological Data. He was the AIBS representative to the Scientific Manpower Commission until his death.

Perhaps, however, his most important administrative duties were those which grew out of his interest in the effective use of scientific manpower during World War II, and these were to accrue to the advantage not only of biologists and other scientists but of the whole nation. During World War I, it will be remembered, Nicholas had been able to volunteer for a particular service in the Medical Corps and to take advantage of his knowledge of bacteriology. During World War II and after, he was involved in the development of national policies that not only permitted the recruitment and the wise utilization of trained personnel but that also allowed scientists to be deferred from military duty when this was, in the long run, to the benefit of the nation. His sense of proportion and of the fitness of things was of great influence in the framing of such policies.

He acted as consultant or adviser to so many bodies concerned with manpower problems, and some of them, such as the National Roster of Scientific and Specialized Personnel, them-

selves operated under so many agencies, that here, once more, as in the case of his scientific work, it is difficult to classify his activities chronologically or systematically. He seems to have begun his work in this area, however, as the National Research Council representative on the National Roster when it was an agency administered by the National Resources Planning Board from 1940 to 1942. He continued in this capacity when the Roster was administered by the War Manpower Commission from 1942 to 1944, and when the Roster moved in 1945 to the Department of Labor, under the U.S. Employment Service, until the Roster as such became inactive in 1947. From 1953 until his death he was a member of the Scientific Manpower Commission and held a number of officerships on the Commission: the Vice Presidency in 1954, the Presidency in 1956 and 1957, membership on its Executive Committee in 1955 and again in 1962. In 1963 the U.S. Department of Labor appointed him to the National Manpower Advisory Committee's Subcommittee on Research, constituted to make comments and suggestions on the programs both of the Department of Labor and of the Department of Health, Education, and Welfare. This subcommittee met only once during the period intervening between Nicholas' appointment to it and his death.

Thus, although the Roster under its original name became inactive in 1947, Nicholas never ceased to be involved in the formulation of policies concerning personnel, and he carried on numerous other activities in this area in addition to those already specified. From about 1944 until the time of his death he was a member of the Advisory Committee to the Office of Scientific Personnel of the National Academy of Sciences-National Research Council. In 1951 another committee of the National Academy of Sciences was created to advise on scientific personnel in the armed forces; this served also as a Committee on Manpower advisory to the Office of Scientific Personnel;

Nicholas was a member of this committee from the time of its establishment until it was disbanded in 1956. In 1956 an Advisory Committee on Scientific Manpower was appointed to review and guide the activities of the Academy and the Research Council related to broad problems of scientific personnel; Nicholas served on this committee from 1956 until his death.

During World War II he also was a member of the National Committee on Physicists of the War Manpower Commission, and a member of the War Emergency Committee of the American Society of Zoologists and of the American Association of Anatomists. His membership on the American Institute of Biological Sciences Committee concerned with utilization of scientific personnel has already been mentioned. Nicholas was also a member of the Selective Service committees which were created in 1948 to advise the Director of Selective Service on deferment policies following reenactment of the Selective Service Act in 1948. These committees, according to the Director of the Office of Scientific Personnel, created the concept of state committees advisory to the state directors of Selective Service; they played an important role in the development of Selective Service policies that were so important during the Korean conflict and after it, and that prevailed until the Vietnam engagement was well under way.

It is surely evident from this enumeration, long but probably incomplete even so, of the academic and professional activities in which Nicholas indulged that his wells of energy, physical and mental, were deep indeed. One of his particular flairs in life was for the enjoyment of work, and he worked with gusto. He enjoyed studying as a youngster, he enjoyed working as an adult, but throughout his life he carried on pursuits seemingly unrelated to the experiences of his work.

Some of his nonprofessional diversions were intellectual. Sundays, for clergymen's children, are not days of games and play,

and Nicholas, who had learned to read early, before attending school, spent his Sundays with books when he was a child. He read avidly, and remained always a rapid and voracious reader on a wide variety of subjects.

He had been received into the Lutheran church at the age of twelve, and unlike a number of other scientists who as sons of clergymen have turned against religion itself, he always retained his religious beliefs to some degree. At the time of his death he was a member of the Board of Deacons of the Church of Christ in Yale University; the Reverend Sidney Lovett, for many years a greatly beloved University Chaplain at Yale, said of him that "in him the disciplines of Science and the insights of Religion were finely joined."

Other of his diversions and accomplishments were musical. He had an excellent ear and a beautiful bass voice. His musical education began during his third year in high school when he started piano lessons, which he continued for two years but never really enjoyed. He meantime had found an old three-quarter-sized violin, and began to fiddle. This appealed to him more; his mother drilled him, and he soon became a member of the Sunday School orchestra. She also arranged for him to have lessons from a teacher at the Harrisburg Conservatory of Music, and he eventually also played in the high school orchestra. He seems, however, to have abandoned playing the violin, and although toward the end of his life he took up the violoncello, his principal musical interests during the major part of his life were vocal. His voice changed early, and he became a member of a church choir in 1910. He continued to participate in group singing throughout his life. During his college years he sang in various college and church choirs and in the glee club. He later sang in the chapel choir at Yale and with the University Glee Club of New Haven, of which he was Vice President in 1962-1963; he was elected President for the year 1963-1964. He

placed little emphasis, when he spoke of his musical activities, on his talents; singing came to him as naturally as breathing.

But for all that he could and did do, his most important talent may have been in being what he was. Mercurial, and thus stimulating, yet somehow always steadfast, he made an exceptionally strong emotional impact on those who were associated with him. Imaginative and intensely energetic as a scientist, he acted with common sense in the extrascientific world. He lived for his work, yet he lived his life, outside the laboratory as well as within it, with a particular exuberance. He had so much life, and lived it so fully, that it even now seems scarcely credible that his days have ended. Members of the National Academy of Sciences, those of his colleagues at Yale who took the full measure of his stature, those who benefited from his judgment at Yale and in the wider world, and his friends honor him in remembering him, as he did them honor by giving them so warmly and so generously of the fruits of his many labors.

Jedes Leben sei zu führen,
Wenn man sich nicht selbst vermisst;
Alles könne man verlieren,
Wenn man bliebe, was man ist.

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KEY TO ABBREVIATIONS

- Am. J. Anat. = American Journal of Anatomy
Am. J. Physiol. = American Journal of Physiology
Am. Naturalist = American Naturalist
Am. Phil. Soc. Year Book = American Philosophical Society Year Book
Am. Scientist = American Scientist
Anat. Rec. = Anatomical Record
Ann. Rev. Physiol. = Annual Reviews of Physiology
J. Biol. Phot. Assoc. = Journal of the Biological Photographic Association
J. Comp. Neurol. = Journal of Comparative Neurology
J. Exp. Zool. = Journal of Experimental Zoology
Proc. Nat. Acad. Sci. = Proceedings of the National Academy of Sciences
Proc. Soc. Exp. Biol. Med. = Proceedings of the Society for Experimental Biology and Medicine
Trans. Connecticut Acad. Arts Sci. = Transactions of the Connecticut Academy of Arts and Sciences
Yale J. Biol. Med. = Yale Journal of Biology and Medicine
Yale Sci. Mag. = Yale Scientific Magazine

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Carl Heumann

CARL NIEMANN

July 6, 1908–April 29, 1964

BY JOHN D. ROBERTS

IN AN ERA when professors of scientific subjects are under heavy pressures to contribute simultaneously to research, to education, to the advancement of the scientific society, and to the advancement of society in general, the life of Carl Niemann provides a fine example of how individualism, tempered and disciplined by responsibility, can lead to an enviable balance of achievements—so much so as to merit description of the man as an “ideal” academic scientist.

Carl Niemann was born on July 6, 1908, in St. Louis, Missouri, the only child of Julius Henry and Ella Danner Niemann. Julius Niemann was a salesman for a local brewery, a large man, fond of hunting, and a strict disciplinarian. Carl took after his mother, to some degree in appearance, but more in having an outwardly quiet temperament, an instinctive feeling for others, and a high degree of manual dexterity. The available accounts indicate that his early childhood was not unusual, although it seems that visits to the households of his mother's relatives often provided a welcome relief from the strictness with which his father regulated his homelife. On entering Cleveland High School in St. Louis, Carl was so competent at carpentry that he started in the manual training rather than the college preparatory curriculum. His orientation changed when he took

high school chemistry, as a result of which his interest was kindled to the point that he set up a rather sizable laboratory in his home basement. In this he was encouraged by his uncle, George Danner, who procured chemicals and supplies for him from the Meyer Brothers Drug Company. A crisis seems to have occurred after his graduation from high school because his father wanted him to enroll in business school to learn accounting and this was far from his own inclinations. The intervention of his uncle seems to have been decisive in helping him move toward chemistry by way of arranging with Edgar Queeny, president and founder of Monsanto Chemical Company as well as a onetime employee of Meyer Brothers Drug Company, for a trial position as chemist in the Monsanto laboratories. During this period Niemann worked in his basement laboratory on weekends and attended Washington University night school. In 1926 he entered the University of Wisconsin, but when his father died in 1928 he came back to St. Louis to work another year for Monsanto while helping his mother adjust to their new circumstances.

The research career of Carl Niemann began after his return to Wisconsin in 1929 when he started his association with Karl Paul Link. The next five years, in his progression from undergraduate, to graduate student, to postdoctoral fellow, led to thirteen papers, all concerned with carbohydrates—being principally fundamental studies of the chemistry of uronic acids, substances which occur naturally along with cellulose and starch. Although he and Link were rather different types, they worked very well together and formed a lasting friendship. The last year of Niemann's Wisconsin career began with his marriage to Mary Parkhurst on August 15, 1934. Then, in 1935, the young couple moved to New York for a two-year stay at the Rockefeller Institute. Here, in collaboration with Max Bergmann, Niemann started the work on proteins and enzymes which was to become his major scientific interest.

It was an exciting time in protein research. Accurate analyses of the amino acid compositions of reasonably pure proteins were just being obtained, and Niemann and Bergmann were imbued with the notion that there could be a high degree of regularity in protein composition based on simple numerical rules. Thus, in several papers published between 1936 and 1939, they advocated that the numbers of amino acids in proteins might in general follow a stoichiometric law of $2^m \times 3^n$ where m and n are positive whole numbers. The published discussion of a paper given by Niemann on this subject at a 1938 Cold Spring Harbor symposium indicates substantial differences of opinion on the validity of the hypothesis, with Wrinch, White, and Mirsky in opposition. Niemann's replies were at all times beautifully phrased but had to be largely defensive because of the scarcity of accurate composition data.

Niemann's last general paper on protein structure was published with Linus Pauling in 1939 and was a scathing (and effective) critique of the "cyclol" theory of protein structure proposed by F. C. Frank in 1933 and developed subsequently in detail by D. M. Wrinch. The general question was whether peptide chains are bonded to one another by hydrogen bonds or by addition of N-H of one chain to C=O of another; and the particular question was whether X-ray diffraction data obtained by Dorothy Crowfoot (Hodgkins) on zinc insulin could be interpreted as was done by Wrinch and Irving Langmuir, in favor of a cyclol-type structure. In the paper with Pauling, not much is said about the stoichiometry of amino acids in proteins and one has the feeling that Niemann was no longer prepared to defend the thesis that his earlier hypothesis was a general one. Pauling and Niemann did point out that, if the number of the residues along the main chain in simple proteins turns out to be 288 ($2^5 \times 3^2$), the reason must be biochemical in origin rather than deriving from a physical factor providing special stability for 288 residues.

After two years at the Rockefeller Institute, Niemann accepted a position as assistant professor of organic chemistry at the California Institute of Technology but took a leave of absence to spend a year at University College Hospital Medical School in London as a Rockefeller Foundation Fellow. Then, in 1937, he and his wife moved to Pasadena, where he was to spend the rest of his life.

By the time Niemann came to Caltech, the Gates and Crellin Laboratories, under the leadership first of A. A. Noyes and then of Linus Pauling, had become world famous in physical and inorganic chemistry. Among the professors, besides Pauling, were William N. Lacey, Richard C. Tolman, Roscoe G. Dickinson, and Don M. Yost, and at Niemann's rank as assistant professor were also Arnold O. Beckman, Richard M. Badger, and Ernest H. Swift. Up to 1937, almost all of the teaching and research in organic chemistry was carried on by Howard J. Lucas, who was an excellent researcher and a leader in America in developing the new field of physical organic chemistry. Niemann's appointment was made possible by a special grant from the Rockefeller Foundation to provide more balance in the organic area and fitted in with Pauling's growing interest in molecular biology.

During his first few years at Caltech, Niemann carried a moderate teaching load—five one-term courses each year—and began a program of research on compounds related to thyroxine, the thyroid principle; on the structure of sphingosine, a prevalent component of nerve tissue; and on histamine action. The work in thyroxine and histamine action was aimed at discovering the structural requirements for biological activity.

Shortly after the United States entered World War II, Niemann was made an official investigator for the National Defense Research Council and in partnership with Ernest H. Swift carried on an extensive research program on analytical methods for

chemical warfare agents. Particular emphasis was placed on procedures for rapid identification of new war gases which it was feared might be suddenly introduced on the battlefields or in cities by aerial attack. The work was carried on for Division 9 of the NDRC and was highly successful although, fortunately, its practical application in wartime work was primarily for identification of war gases manufactured by the Germans and uncovered by our invasion forces but not actually used in combat. The later stages of the NDRC work involved intensive field trials and Niemann spent much time at testing stations in Panama and Florida. Finally, in 1944 he served for several months as consultant to the General Headquarters Staff in the Southwest Pacific area. He was awarded a Presidential Certificate of Merit for his contributions to the war effort.

The postwar phase of Niemann's work was marked by the beginnings of his research on the action of the protolytic enzyme, α -chymotrypsin, one of the most fascinating, and as yet unsolved, problems of organic chemistry. The choice of this enzyme for study was not due to luck or a haphazard guess. Niemann recognized that α -chymotrypsin, although an extremely complex substance of unknown structure, was at least relatively stable and reproducible with a rather nonspecific mode of action. The relative nonspecificity of action meant that wholly artificial substances could be "digested" by the enzyme, and through measurements of the rates of these digestive-type processes, in relation to the structures of the substrates supplied, it was hoped to discover how the enzyme worked. The procedure was an essentially topological one and, in fact, something like trying to deduce the eating apparatus and habits of a creature living in a cave by throwing in variously shaped pieces of different kinds of foods and seeing which are consumed most readily, if at all.

The work in α -chymotrypsin led to more than ninety research

publications in about fifteen years, one of the most important being a summary article published in *Science* in March 1964. Niemann's view of his achievements in the research on chymotrypsin was a characteristically modest one and was laid out in the conclusion of his article in *Science*:

"In 1906 Fischer described the approach to elucidation of the chemical synthesis of a protein in terms that might be applied today to elucidation of the mechanism of action of an enzyme. The approach, he said, is that of a pedestrian who 'seeks his way step by step with careful attentiveness and who must examine many roads until he has found the right one. On his long and troublesome travels, not only does he learn to recognize completely the geography and topography of the country, but also he becomes intimate with the tongue and culture of its inhabitants. When he finally has reached his goal, he is able to locate himself properly in every corner of the country and this will be possible for other people also if he writes a book about it.' As for elucidation of the mechanism of action of α -chymotrypsin, the pedestrian has barely begun his journey, and this review is his first letter home."

The spirit in which Niemann's work on chymotrypsin-induced reactions was carried out was almost as important as the specific achievements. There was no striving for a quick, clever solution to the mechanistic problem but instead the approach was meticulously designed to give a broad spectrum of results with which theoretical approaches could be tested. The emphasis was on the determination of accurate values of experimental reaction-rate parameters characteristic of a variety of substrates and, particularly in the early stages, there was little attempt to discuss the mechanistic implications of the results.

The typical thoroughness of Niemann's work is illustrated by a statement from his first major paper with Huang on chymotrypsin in 1951:

"In this study, consideration was given to (a) the possibility of interaction of the enzyme with the buffer components, (b) the dependence of the activity of the enzyme on the pH of the reaction system, (c) the possibility of inhibition of the hydrolytic reaction by one or both of the hydrolysis products, (d) the development of suitable rate expressions and the determination of rate constants and (e) the possibility of the inhibition of the hydrolysis of the above substrates by their D-antipodes."

Later papers developed other important aspects of chymotrypsin methodology, including ways of analyzing the rate curves and the effects on the rates of salts, nonaqueous solvents, inhibitors, dimerization of the enzyme, and even adsorption of the enzyme on the walls of the containers. These studies were especially significant in showing the limitations which have to be placed on comparisons drawn between results collected under different experimental conditions.

Niemann had very clear ideas about the practice of laboratory work. Although several generations of students used essentially the same instruments and methods, he refused to set up standard procedures. It was his conviction that absence of standard procedure was some protection against determinate errors which otherwise might avoid detection. In addition, he would not hire technicians to do any of the work although some of the rate measurements seemed repetitive to his co-workers.

A considerable breakthrough came in 1960 with the discovery that the normal relative stereospecificity of chymotrypsin which markedly favors hydrolysis of L- over D-amino acid derivatives could be overturned by suitable substituent groups. The consequences of this discovery were developed in considerable detail by Niemann and Hein. Of particular importance was the concept of "wrong-way" binding which emphasizes that enzyme-substrate complexes may be combined at the active site in such a way as to be unproductive for the normal hydrolysis reaction.

The value of this hypothesis lies in its ability to explain why there has been no clear correlation between how firmly substrates of different structures are bound by the enzyme and the rates at which they hydrolyze. The hypothesis was further developed by assuming that relative affinities could be assigned to the substituent groups which are found in different substrates for various interaction sites in the enzyme—the aim being to permit prediction of the favored orientation of a substrate at the active site which, of course, might or might not be favorable for hydrolysis.

These ideas were not developed without opposition, and many of the Niemann papers either involved long battles with unsympathetic referees or contained detailed defenses of ideas attacked by critics. In these, Niemann was unfailingly courteous, but his command of language was such that a courteous reply could be incisively, if not stingingly, to the point.

The professional stature of a scientist can usually be established rather well from his published work provided reasonable account is taken of the mood of the times in which it was accomplished. Personal stature is often more ephemeral, having to be based on testimony deriving from recollections of close, or not so close, associates in the absence of soul-searching autobiographical material. Carl Niemann was not one who left a mass of heterogeneous memorabilia for posterity to use in constructing a personal image. Although he died wholly unexpectedly, just before he was to depart on a European vacation, his correspondence files were almost completely bare, it being his custom after answering a letter to consign both the letter and the carbon of his reply to the wastebasket. In striking contrast, he had very complete files of his reprints and manuscripts, often including early as well as final versions of his papers. There can be no clearer message of what the man himself regarded as important.

Carl Niemann in middle life was a good-looking man of somewhat more than medium height with an unostentatious, ur-

bane appearance and an unusually pleasant and calm speaking voice. He was not a man who bustled or became outwardly excited and yet, in talking with him, one could sense a considerable inner tension and drive. He was a highly effective person. His less efficient colleagues never ceased to marvel at how he worked day after day (and many evenings) with his office door wide open. He always had a cheerful welcome for anyone wishing to consult with him, but as soon as the business at hand was over, one sensed he was, while remaining completely polite, beginning to think about the next phrase of the manuscript before him. He was a craftsman of scientific writing; each paper was written, rewritten, and rewritten again before he was satisfied. He usually worked on one thing at a time—in the midst of an important paper, letters went unanswered, forms remained uncompleted; only the business one could transact with him orally made much headway.

A picture of Niemann as a strongly independent, research-centered scientist would be quite incomplete. He was tenaciously independent, even if quietly so, but at the same time he believed that independence in an academic environment carried with it the obligation to be responsible, and in every commitment he took on he was always completely responsible. He disliked administrative work but he was a fine administrator, having for many years heavy responsibility as the Chairman of the Divisional Graduate Committee. It was administration on his own terms—he had independence of action in an important but limited area and, furthermore, the work was largely seasonal, leaving him with much free time for his research. In addition, he was in almost constant demand for service on Caltech committees and he was exceptionally effective in such work, not only because of his sensitivity to other people's ideas and difficulties, but also because of his rare sense of humor which was particularly useful for reducing arguments to their proper perspective.

Niemann was elected to the National Academy of Sciences

in 1952 and was very interested in its affairs. He attended almost every annual meeting and served during the last two years before his death as Chairman of the Chemistry Section. For ten years he was a member of the Editorial Board of *Organic Reactions*, an annual collection of review chapters on important synthetic methods. It was a tedious job but was suited to his habits by virtue of being seasonal and was rewarding through association with the other editors, particularly Roger Adams of the University of Illinois, who founded the series in 1942.

Niemann had many interests outside of his scientific work. He read widely and had a detailed knowledge of the history of the Civil War. He loved the outdoors and enjoyed traveling with his wife and two daughters. Under the guidance of his good friend, Carl S. Marvel, Niemann became an ardent and expert birdwatcher, and at one time or another he sighted most of the important species of the land and water birds of the United States.

Carl Niemann was a scholar and a scientist of the first rank. He was devoted to the principles of both academic freedom and academic responsibility. His legacy is a corps of superbly trained and loyal students, a deeper understanding of the chemistry of life, and the many results of his efforts on behalf of the California Institute of Technology. He died very suddenly at the height of his career. His many friends will not soon forget the way in which he enriched their lives and the scientific profession.

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KEY TO ABBREVIATIONS

- Anal. Chem. = Analytical Chemistry
Arch. Biochem. = Archives of Biochemistry
Arch. Biochem. Biophys. = Archives of Biochemistry and Biophysics
Biochim. Biophys. Acta = Biochimica et Biophysica Acta
Cold Spring Harbor Symp. Quant. Biol. = Cold Spring Harbor Symposia on Quantitative Biology
J. Am. Chem. Soc. = Journal of the American Chemical Society
J. Biol. Chem. = Journal of Biological Chemistry
J. Ind. Eng. Chem., Anal. Ed. = Journal of Industrial and Engineering Chemistry, Analytical Edition
J. Nat. Cancer Inst. = Journal of the National Cancer Institute
J. Org. Chem. = Journal of Organic Chemistry
Proc. Nat. Acad. Sci. = Proceedings of the National Academy of Sciences
Proc. Soc. Exp. Biol. Med. = Proceedings of the Society for Experimental Biology and Medicine

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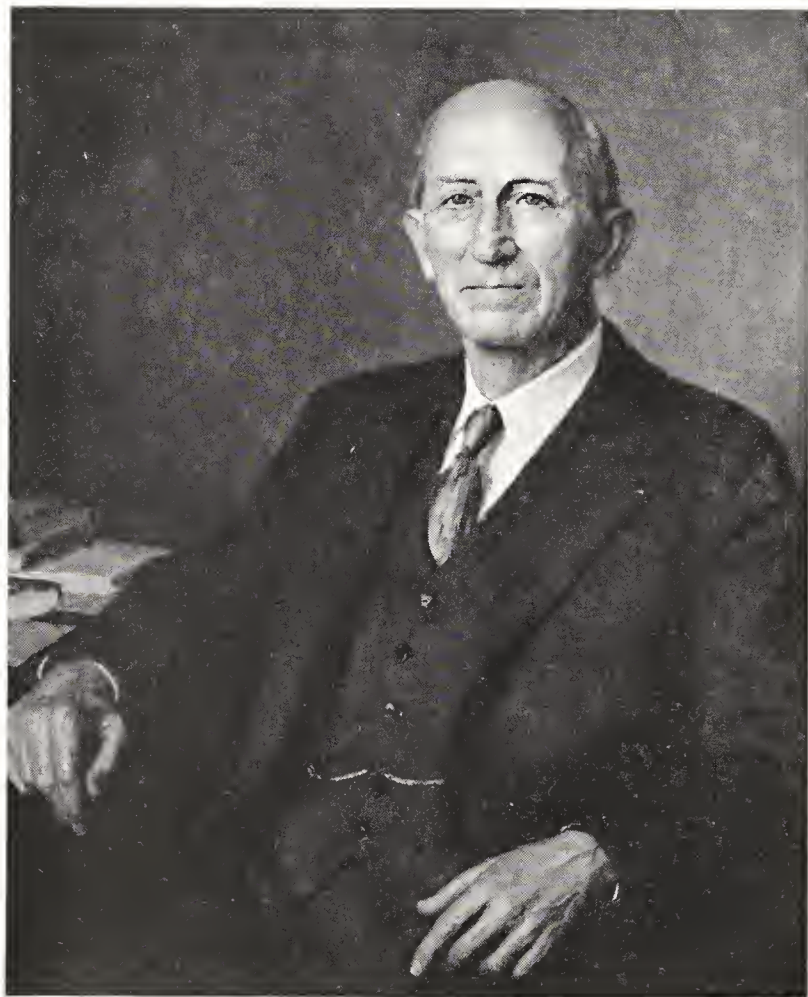
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Philip A. Shaffer

PHILIP ANDERSON SHAFFER

September 20, 1881–December 4, 1960

BY EDWARD A. DOISY

PHILIP ANDERSON SHAFFER was born in Martinsburg, West Virginia, on September 20, 1881. His ancestors on his mother's side had lived in the Shenandoah Valley from pre-Revolutionary War days and on his father's side from 1795. His grandfather, Philip Anderson, was a physician who studied medicine in Philadelphia and upon completion of his studies received a diploma signed by Dr. Weir Mitchell. Apparently inspired by Dr. Mitchell, he stayed in Philadelphia several years for more experience and training and subsequently took a year of graduate work at the University of Virginia. He returned to the Shenandoah Valley, married Mary Dyer, and engaged in the practice of medicine.

The eldest daughter of Dr. Philip Anderson, Hannah, had attended a finishing school and at the time of her marriage to Joseph Shaffer in 1880 she was teaching music in a school in Winchester. The newly wedded couple began married life in Martinsburg, West Virginia, and Philip Anderson Shaffer was born in 1881. It appears that the achievements of his grandfather in medicine, which were recounted in the family circle during his early life, played an important role in his later choice of a profession.

The author is indebted to Mrs. Nan Evans Shaffer for her

authoritative statements regarding the early life of her husband, Philip. She has supplied the following paragraphs, which in a few instances have been slightly altered.

"Philip's father did not have the scholarly background of his grandfather, Dr. Anderson, but possessed a keen mentality and was a well-read person. His personality contained some of the traits we have so admired and enjoyed in Philip Shaffer, a quiet iron-strong gentleness and forthrightness. He had excellent judgment, was consulted by various business interests, was a director of the bank, and, almost incidentally, ran his store that amounted to a wholesale outlet for the area.

"A young clerk in that store was greatly interested in the Shaffer boy, and before Philip Anderson Shaffer was three years old had him plunked on the end of one of the old black-walnut counters learning to read from the *Baltimore Sun* and having great fun doing it. When Philip was of the age to start school he could read almost anything and was terribly bored by school because when the other youngsters were struggling with something new he had nothing new to do. The teacher finally reported to the parents that Philip was a most unsatisfactory child. So he was sent to Miss Betty Wilshire's private school. At Miss Betty's the placing of a pupil depended upon his ability to read: Philip could read anything so he went on being jumped over classes in arithmetic, geography, even spelling, and entered public schools again with the same boredom as before.

"There happened to be a new teacher in the Martinsburg High School, A. B. Carmen, recently graduated from Harvard University. He became acquainted with the boy through contacts other than school, and was convinced that here was a student who, if not superior, was at least different, and he 'took' him on, tutored him, engineered him into and out of high school by intricately shifted courses, and helped him enter the University of West Virginia while he was still fifteen years old—a little before his sixteenth birthday.

"In anticipation of entering the University of West Virginia, Philip went to Harper's Ferry to ask the proper officer for cadet appointment. He received it, and with great pleasure told the family and Carmen what he had done. When he entered the University it was in a cadet uniform. Then came the Spanish-American War. Philip, a cadet, was eager to fulfill his obligations and march off to war with other students. His age, however, made it necessary to submit written permission from his parents; this they refused to give, very much to Philip's 'humiliation and chagrin,' and he remained in college."

At the beginning of his junior year he encountered a newly appointed teacher of chemistry, Otto Folin, who had just returned from his studies abroad. Dr. Folin had become interested in medical chemistry but could not find a suitable position in this type of work in the United States. This was very fortunate for Philip Shaffer, as their association led him into biochemistry under the tutelage of Dr. Folin and to an enduring friendship.

Dr. Folin gave courses in quantitative analysis and physiological chemistry. Philip Shaffer took both courses and was particularly interested in the latter. Since equipment was meager many of the experiments were omitted. Apparently this was a stimulus to thinking and theorizing, and Philip must have demonstrated that he was the most promising of the students because he was chosen to accompany Dr. Folin to a newly established position in the McLean Hospital in Waverly, Massachusetts.

The McLean Hospital had a forward-looking director, Dr. Cowles, who not only established this research laboratory to learn if body chemistry had a connection with mental health but also provided an extensive library for the staff. A professional librarian, Nan Jefferson Evans, who later became Mrs. Shaffer, was employed.

Dr. Folin, with his young associate, Philip Shaffer, who had just graduated from the University of West Virginia at the age of nineteen, undertook the study of chemical changes in relation

to mental health but soon found that the available analytical methods were not satisfactory. The preliminary work consisted of improvements in procedures for the determination of urinary constituents; the improved methods were used in the work which was published in 1904: "Some Metabolism Studies, with Special Reference to Mental Disorders."

During his work at McLean Hospital Philip demonstrated ability in research, but in spite of his pleasant association with Dr. Folin he decided to take the year of residence required by Harvard University to obtain the doctorate. Therefore, at the opening of Harvard in the fall of 1903 he left the hospital and in June 1904 received the first Ph.D. degree in physiological chemistry ever given by Harvard University. The next day he was married to Nan Jefferson Evans, the librarian of McLean Hospital.

Several industrial positions with good salaries were offered him but only one research place, in the Loomis Laboratory in New York City, a part of Cornell Medical College, Department of Experimental Pathology. The salary of \$85 per month was not attractive but his bride, knowing that his heart was in medical research, encouraged him to accept it. The confidence which she had in his investigative ability was justified, as demonstrated by his appointment to the chair in physiological chemistry at Washington University School of Medicine (St. Louis) six years later.

Of the publications from the Loomis Laboratory, those on the study of the metabolism of typhoid patients were of the most interest. After some discouragement, Philip finally obtained the cooperation of a physician, Dr. Warren Coleman. Together they demonstrated that by feeding a high caloric diet containing a liberal quantity of protein a positive nitrogen balance could be obtained, thereby preventing the loss of body protein. In some instances patients on this diet actually gained weight.

There were other lifelong results of this time in New York. Since his papers in biochemistry had already attracted attention, Philip was invited to attend the preliminary meeting which was called for the organization of the American Society of Biological Chemists. He was one of the twenty-nine who founded the Society in 1906. As a charter member, he was asked to share in the program of the fiftieth anniversary of the Society.

Undoubtedly it was his notable work on typhoid fever that brought consideration of his name when, under Robert Brookings' farsighted direction, a complete reorganization of Washington University Medical School was being undertaken, and undoubtedly it was the galaxy of young and open minds he would be associated with that induced him to accept the offer to head the Department of Biochemistry and to take his place on the "Executive Faculty" (an entirely new idea). Philip Shaffer was the youngest of the new departmental chairmen—he was twenty-nine at the time of his appointment. In September 1910 he moved his family to St. Louis and joined wholeheartedly in the development of the Medical School. To this end he thereafter devoted his life, often sacrificing his personal preferences or scientific advancement in taking on administrative burdens to maintain the standards which the original Executive Faculty had worked so hard to establish.

Robert Brookings wisely decided to assemble an excellent faculty before undertaking the building of physical facilities for the developing Medical School. Philip Shaffer, together with his staff, Dr. Walter Bloor and Dr. W. McKim Marriott, the newly organized Department of Biochemistry, began teaching in 1910 in the old buildings. Judging by the papers published, the surroundings did not interfere with research.

The Executive Faculty continued planning for the new buildings, which were soon (1912) under construction and were completed in 1914. One of the first national scientific gatherings

held in them was the ninth meeting of the American Society of Biological Chemists, which was held on December 27-30, 1914. Philip, who was the secretary of this scientific body, presented a paper during the meeting.

In 1915 he became dean of the Medical School, an office which he held until 1919. When the United States entered World War I, he felt the call of duty and served as a major in the Food Division of the Sanitary Corps from 1917 until 1919. The bibliography below bears eloquent testimony to his absence from the laboratory and the result of his return to scientific work during the latter part of 1919.

During his first term as dean, several brilliant young men were brought to Washington University to head departments in the full-time program in which the Medical School had embarked. Dr. W. McKim Marriott, formerly in the Biochemistry Department, was appointed to the chair in pediatrics in 1917, and Dr. Evarts Graham to the chair in surgery in 1919.

Although Philip's first term as dean ended in 1919, he continued as a member of the Executive Faculty and later served as dean from 1937 to 1946. During his entire association with Washington University, Philip was actively engaged in the quest of promising young men for the Medical School. He played an important role in the appointments of Dr. David P. Barr and, subsequently, Dr. W. Barry Wood in medicine, Dr. Willard M. Allen in obstetrics and gynecology, Dr. E. K. Marshall, Dr. Herbert S. Gasser, and later Dr. Carl Cori and Dr. Gerty Cori in pharmacology. Two medical students who subsequently became chairmen of departments, Dr. Alexis F. Hartmann in pediatrics and Dr. Carl V. Moore in medicine, were encouraged to devote their lives to teaching and investigation by their accomplishments in research in Philip's department during their undergraduate days in the Medical School.

In addition to his outstanding service to Washington Univer-

sity in its development of the Medical School, Philip Shaffer was very active in the affairs of the American Society of Biological Chemists, Inc. He served the Society with such distinction in several offices—councillor for six years, secretary for two, vice president for two, president for two, and many years as chairman of the Editorial Board—that in later years he was frequently referred to by his contemporaries as the Dean of American Biochemistry. At the meeting celebrating the fiftieth anniversary of the founding of the Society, he presented a paper, by invitation, entitled “Origin and Development of the American Society of Biological Chemists.” This provided a fine opportunity to review the accomplishments of biochemistry, a review that strongly supported the prophetic statement he had made in his introductory lecture to the freshmen medical students in 1914: “Most of the advances in medicine will come through biochemistry.”

Philip Shaffer's first scientific paper, “Ueber die Quantitative Bestimmung der Harnsäure im Harn,” was published in 1901 with Otto Folin as the senior author. Most of his subsequent papers for the next few years were studies based on the analysis of urine. While he was at the Loomis Laboratory of Cornell University Medical School his studies led to his proposal of the revolutionary idea, at that time, of a high caloric diet for typhoid patients. Together with Warren Coleman, he clearly demonstrated that this was an important innovation in the care of typhoid patients. After he moved to St. Louis, his publications dealt mainly with methods for the determination of components of urine and blood. One of these, on the determination of blood sugar, was the cause of his continuing interest in the subject and led to a consideration of diabetes. A series of papers based both on balance studies previously published by others and on his own experiments was published under the general heading “Antiketogenesis.” With his interest in the metabolism

of diabetes already developed he was ready, after the announcement of the preparation of insulin by Banting, Best, Collip, and Macleod, to see this new product used in the Pediatrics Department of Washington University School of Medicine in the treatment of a diabetic baby. Since insulin could not be supplied by the Toronto group, he hurriedly started the extraction of it from beef pancreas. The first preparation proved to be effective in tests on animals and relieved the severe symptoms of diabetes in the baby. Continuation of the extraction, purification, and characterization of insulin led to (1) the use of sufficient strong sulfuric acid to ensure inhibition of tryptic activity, (2) the discovery of isoelectric precipitation of insulin for its purification, and (3) recognition of the protein nature of highly purified insulin.

The deanship and other administrative duties again interfered with scientific work so much that in his later years publication was less frequent. However, he was intrigued by oxidation-reduction reactions and published under the general title "Equi-valence Change Principle." He found that reactions between ions in which valence changes were not equal proceeded very slowly unless catalyzed by stepwise transfer of electrons.

Philip was a kind, thoughtful, and considerate man. His analytical mind enabled him to come promptly to a conclusion which he expressed forcefully. On one occasion, during a discussion on the changing of the time of the meeting of the American Society of Biological Chemists from winter to spring, he listened attentively to the other speakers. Then he finished the discussion by saying, "How can anyone be opposed to the change; this is a group of experimentalists. If we make a change and don't like it, we can revert to the winter meeting." The vote for the change was affirmative and after forty years the meetings are still held in the spring.

The writer and Philip were close friends for forty years.

Each of us discussed many problems with the other; this was beneficial to me because of Philip's capacity to think clearly. In fact, his friendship was so enjoyable that almost invariably my wife and I helped him celebrate his birthday by a call to deliver a small token of our affection.

On many occasions Philip stated, "I am enjoying what I am doing so much that if I could afford to do so I would be happy to pay Washington University for the privilege of doing just what I am doing."

In 1946 Philip Shaffer was given the title Distinguished Service Professor of Biological Chemistry by Washington University, a title amply warranted by his accomplishment in biochemistry and his devoted service to Washington University. He became an emeritus professor in 1952 but continued his laboratory work as long as his health permitted. After several months of hospitalization, Philip A. Shaffer passed away on December 4, 1960.

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KEY TO ABBREVIATIONS

Am. J. Physiol. = American Journal of Physiology

J. Am. Chem. Soc. = Journal of the American Chemical Society

J. Biol. Chem. = Journal of Biological Chemistry

Washington Univ. Med. Alum. Quart. = Washington University Medical Alumni Quarterly

Z. physiol. Chem. = Hoppe Seyler's Zeitschrift für physiologische Chemie

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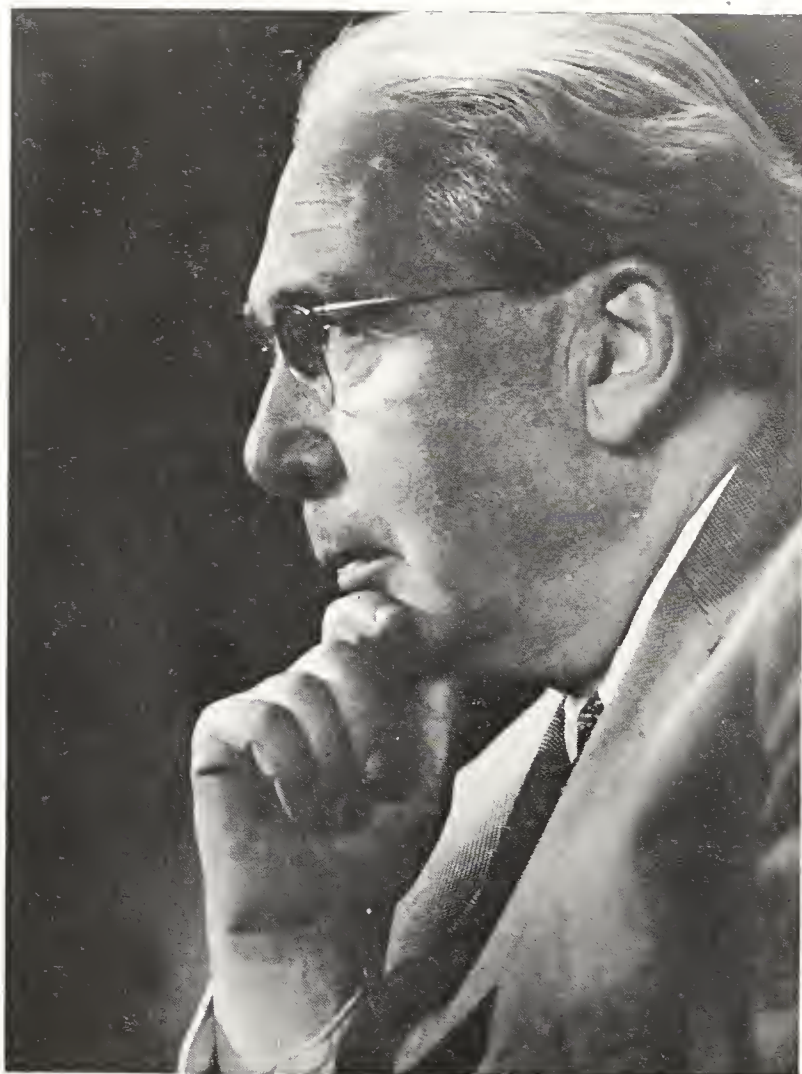
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February 11, 1898–May 30, 1964

BY EUGENE P. WIGNER

DURING A LONG LIFE among scientists, I have met no one with more imagination and originality, with more independence of thought and opinion, than Leo Szilard. As a scientist, he contributed significantly to statistical mechanics, to nuclear physics, and to biology. As an engineer, he invented a method for pumping liquid metals, had a large share in the establishment of the nuclear chain reaction, and was one of the first to recognize the variety of purposes for which it can be used. As a citizen, he contributed as much as anyone—perhaps more than anyone else—to the undertaking by the United States of a large effort toward the exploitation of the fission process, leading to nuclear weapons. As a citizen, he founded at least two associations to influence the policies of the country which adopted him. He did thereby influence those policies and also the thinking of many of his colleagues. He wrote a charming little book, *The Voice of the Dolphins*, which was translated into six languages.

Szilard was born in 1898 in Budapest, Hungary. He seldom spoke about his parents but when he did his affection for his mother rarely failed to come through. After completing his high school studies in Budapest, he entered the Institute of Technology (Muegyetem) of the same city, but his studies were

interrupted by World War I. He served as an officer in the Austro-Hungarian army but never saw action. A few days before the collapse of the front, he returned to Budapest. After the war, he continued his studies at the Institute of Technology (Technische Hochschule) of Berlin. He was, first, a candidate for the Electrical Engineer's degree, but his interest gradually turned toward theoretical physics. This writer first met him at the Institute for Physical Chemistry of the Technische Hochschule—during a brief flirtation of Szilard with this subject.

As soon as it became clear to Szilard that physics was his real interest, he introduced himself, with characteristic directness, to Albert Einstein. I believe it was largely Szilard's doing that Einstein gave a seminar on statistical mechanics at which, in addition to Szilard himself, several of his friends, including D. Gabor and the present writer, participated. Einstein showed great virtuosity not only in recognizing the truly important unsolved questions but also in projecting a picture of the accomplishments and of the spirit of the theory. The seminar was a unique experience for most participants; it also inspired, I believe, Szilard's doctoral dissertation (1922).

Szilard's dissertation, and a companion paper which was published in 1929, contain several of the ideas which form the basis of present information theory. In spite of the obvious significance of these ideas, Szilard did not feel fully at home in theoretical physics. He felt that his skill in mathematical operations could not compete with that of his colleagues, and he spent most of his time experimenting with X-rays in collaboration with H. Mark, and pursuing some of his engineering ideas.

As a result of Hitler's assumption of power, Szilard left Berlin in 1933 and moved to England. It was here that he first engaged in semipolitical activities. With the same directness which characterized so many of Szilard's actions, he approached Sir William Beveridge and induced him to found the Academic Assist-

ance Council. The purpose of this organization was to help scientists who had to leave Germany because of non-Aryan (usually Jewish) descent to settle in other countries and to help them financially to overcome the initial difficulties. The funds were provided partly by donations, partly by a self-imposed tax paid by refugees from Germany who already had found positions elsewhere. The story of the Academic Assistance Council was described more completely in Edward Shils's memoir of Szilard, published in the December 1964 issue of *Encounter* (page 35).

Szilard's move to England also marks the beginning of his active interest in nuclear physics. He discovered, in London, the *modus operandi* which suited his talents best: he teamed up with a younger colleague, in this case T. A. Chalmers. The ideas conceived jointly were executed by this younger man, leaving more time for Szilard's imagination to roam over as yet uncharted territories. Some, at that time, very important measurements with slow neutrons date from this period, as does also the technique whereby nuclei ejected from a compound after absorbing a neutron are chemically separated. This technique—the Szilard-Chalmers process—is now widely used.

Unquestionably, the most important event in Szilard's life took place in England. This was the growth, in his mind, of the conviction that a nuclear chain reaction was possible and that a nuclear bomb could be developed on the basis thereof. The original basis of Szilard's conviction proved to be erroneous, but he held on to his idea tenaciously and it indeed came to fruition when nuclear fission, discovered in 1939, provided the missing key. Szilard was probably the first to suggest that neutron emission might accompany the fission reaction, that the number of neutrons emitted in the process might exceed 1, and that, as a result, it should be possible to establish an energy-producing chain reaction.

Szilard, in his own words, fell in love with England almost at first sight. Nevertheless when, as a result of the Munich agreement, the future of England appeared to be dubious to him, he moved permanently to the United States (1938; he was naturalized in 1943). At first, Szilard worked at Columbia University. As soon as he heard about nuclear fission, he started to determine those characteristics of this process which might render it possible to establish a chain reaction. These experiments were carried out in collaboration with W. H. Zinn, and simultaneously with similar experiments by Anderson and Fermi. The success of these experiments ensured Szilard a prominent place in the laboratory which was established in 1942, under A. H. Compton's leadership in Chicago, for the purpose of establishing such a chain reaction and making plans for a chain-reacting unit of high output. Indeed, "the General," as Szilard was called in the project by friend and foe alike, exerted an important influence not only by his many technical ideas—few of which were actually used—but principally by his foreseeing future problems and suggesting measures to overcome them. Nevertheless, he found the work at the "Metallurgical Laboratory" (the code name for Dr. Compton's project) frustrating.

As soon as the success of Dr. Compton's project appeared assured, Szilard's interest shifted to the more distant implications of the nuclear chain reaction and of the explosive which this produced. The concept of the breeder, a reactor which produces more fuel than it consumes, was born in these days. The new explosive, several million times more powerful than the old ones, posed also difficult political questions. These occupied him until about 1949 when, at the age of fifty-one, he turned to biology.

In his work in biology, Szilard finally realized his full potentialities. He had security and the freedom of a professor at the University of Chicago, as well as a talented and congenial collaborator, Aaron Novick. The simplicity of the experimental

equipment, together with the relative lack of sophistication of the subject, suited his temperament fully. Each of the seven papers published during the period 1948-1955 contains some new and relevant information. Most important is, perhaps, the development of the "chemostat," which is a device for maintaining a multiplying population of bacteria under conditions not changing in time. Thus, in spite of the multiplication of the bacteria, their number per unit volume remains constant. This device permitted him and Novick, for instance, to study the number of mutations produced in unit time under different rates of multiplication. They found that the rate at which mutations occur is independent of the rate of multiplication (number of cell divisions in unit time). An outcome of this period's work is Szilard's theory of aging which, though much questioned, doubtless has had a widely stimulating influence.

The last few years of Szilard's were again devoted to political activities. Altogether, the *homo scientiarum* can hardly be separated from the *homo politicus* in his case. He was strongly under the influence of Plato and his advocacy of rule by an elite. It was a favorite saying of Szilard's that one stupid person may be right as often as a bright one but two stupid people will be wrong much more often than two bright ones; they should not have as much to say about national policies as the latter. However, his good will toward all, including the stupid ones, was always wholehearted and no one can accuse him of malice. The Ten Commandments published in the German edition of *The Voice of the Dolphins* are inspiring reading.

Szilard was elected to the National Academy in 1961. He received the Einstein award—a prize originated by Lewis Strauss—in 1958 and the Atoms for Peace award in 1959. He died in 1964.

KEY TO ABBREVIATIONS

- Bull. Atomic Scientists = Bulletin of Atomic Scientists
Cold Spring Harbor Symp. Quant. Biol. = Cold Spring Harbor Symposia
on Quantitative Biology
Phys. Rev. = Physical Review
Proc. Nat. Acad. Sci. = Proceedings of the National Academy of Sciences
Zeits. Physik = Zeitschrift für Physik

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